



AGRICULTURAL RESEARCH INSTITUTE
PUSA

PHILOSOPHICAL
TRANSACTIONS,

GIVING SOME

A C C O U N T

O F T H E

Present Undertakings, Studies, *and* Labours,

O F T H E

I N G E N I O U S,

I N M A N Y

Considerable Parts of the W O R L D.

VOL. L. PART I. For the Year 1757.

L O N D O N.

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E R R A T A.

Page 95. line 24. read even the ends of the umbilici.

Page 96. line 5. read exposed the extremity of the umbilicus.

Page 168. line 4. after as dele well as.

Page 328. line 9 from the bottom, for stream read steam.

In the Tables of the Variation of the Magnetic Needle, Anno 1756

Lat. Long.

0 — 70 E. for Var. $3\frac{1}{4}$ W read $2\frac{1}{2}$ W. Read the same in p. 333

15 N 35 W ——— $3\frac{1}{2}$ W ——— $2\frac{1}{2}$ W

5 S 40 E ——— 17 W ——— 18 W

30 S 15 E ——— 18 W ——— $17\frac{1}{2}$ W

35 S 10 W ——— $5\frac{1}{2}$ W ——— 5 W

25 S 45 E ——— $2\frac{1}{2}$ W ——— $2\frac{1}{2}$ W

PHILOSOPHICAL TRANSACTIONS.

- I. *An Account of the Earthquake felt in New England, and the neighbouring Parts of America, on the 18th of November 1755. In a Letter to Tho. Birch, D.D. Secret. R. S. by Mr. Professor Winthrop, of Cambridge in New England.*

Reverend Sir,

Read Jan. 13, 1757. **I** Beg leave to lay before you the best account I am able to give of the great earthquake, which shook New England, and the neighbouring parts of America, on Tuesday the 18th day of November 1755, about a quarter after four in the morning. I deferred writing till this time, in order to obtain the most distinct information of the several particulars relating to it, both here and in the other places where it was felt; and especially the extent of it.

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The

The night, in which this earthquake happened, was perfectly calm and serene. In the evening there was a fog over the marshes bordering on the river Charles, which runs through this town: but this I found intirely dissipated at the time of the earthquake, the air being then quite clear, and the moon, which wanted but 36^h of the full, shining very bright. The earthquake began with a roaring noise in the N.W. like thunder at a distance; and this grew fiercer, as the earthquake drew nearer; which was almost a minute in coming to this place, as near as I can collect from one of my neighbours, who was then on the road in this town. He tells me, that, as soon as he heard the noise, he stopt, knowing, that it was an earthquake, and waiting for it; and he reckoned he had stood still about 2', when the noise seemed to overtake him, and the earth began to tremble under him: but, as I doubted, whether it were so long, I counted several numbers to him as slowly as a clock beats seconds; and then he said, he believed he could have counted half an hundred, at that rate, before the noise and shake came up to him. By his account, as well as that of others, the first motion of the earth was what may be called a pulse, or rather an undulation; and resembled (to use his own comparison) that of a long rolling, swelling sea; and the swell was so great, that he was obliged to run and catch hold of something, to prevent being thrown down. The tops of two trees close by him, one of which is 25, the other 30 feet high, he thinks waved at least ten feet (and I depend on his judgment in this particular, because he judged right of the height of the trees, as I found by actual mensura-

mensuration); and there were two of these great wavings, succeeded by one, which was smaller. This sort of motion, after having continued, as has been conjectured, about a minute, abated a little; so that I, who was just then waked, and, I suppose, most others, imagined, that the height of the shock was past. But instantly, without a moment's intermission, the shock came on with redoubled noise and violence; though the species of it was altered to a tremor, or quick horizontal vibratory motion, with sudden jerks and wrenches. The bed, on which I lay, was now tossed from side to side; the whole house was prodigiously agitated; the windows rattled, the beams cracked, as if all would presently be shaken to pieces. When this had continued about 2', it began to abate, and gradually kept decreasing, as if it would be soon over: however, before it had quite ceased, there was a little revival of the trembling and noise, though no-ways comparable to what had been before: but this presently decreased, till all, by degrees, became still and quiet. Thus ended this great shock. It was followed by another about an hour and a quarter after, viz. at 5^h 29'. This, though comparatively small, was very generally perceived, both as to its noise and trembling, by those who were awake. On the Saturday evening following, viz. the 22d of November, at 27' after eight, there was a third, more considerable than the second, but not to be compared with the first. And on Friday the 19th of December in the evening, exactly at ten o'clock, there was a fourth shock, much smaller than either of the former, though, like them, preceded by the peculiar noise of an earthquake.

The whole lasted but a few seconds ; but the jarring^d was great enough to cause the window-shutters and door of the room, in which I then was, to clatter. The sky was perfectly clear, and there was a very gentle and scarcely-perceptible gale at S.W. These four are the only shocks, that I have been sensible of from the 18th of November last to this date ; tho' more are said to have been felt in other parts of the country to the northward of us.

As to the duration of the great shock, people have differed widely, viz. from 1' to 6 or 7. Our printed accounts have generally fixed it to about 2', or 3 at the most ; but as these were only the uncertain guesses of persons, who had no rule to guess by, no dependence can be had on them. I am well-satisfied, that with us it continued 4', or rather 4' 1-half, taking in the whole of the time, from the first agitation of the earth till it was become perfectly quiet ; tho' the violence of the shock did not last above half so long. This I am assured of, partly from the observations of some gentlemen, who were up, and looked on their watches, when it began and ended ; one of whom tells me it was 4', and another, that it was near 5 ; and partly from my own observations, which were as follow. The preceding noon I had adjusted both my clock and watch to the apparent time, by a meridian line ; and the following noon I found, that the watch had kept time exactly. Being awaked by the earthquake, I lay till the violence of it seemed to be over for the second time, the first abatement happening just after I waked. Till then I forbore to rise, because the agitation was so vehement, that I concluded it would be difficult, if not impracticable, to

to go from the bed to the chimney, without being thrown down; and therefore thought it best not to attempt it. The space of time, in which I lay awake, I cannot think to be much, if any thing, less than 2'. This was the conjecture I formed at that time; though, it being but conjecture, I would not lay very great stress upon it, were it not supported by concurring observations. On the second abatement I rose, and lighting a candle, looked on my watch, and found it to be 15' after four. The shock then was not quite over, but the windows continued rattling for about a minute longer, as near as I can remember; for the shock went off very gradually. As soon as I had looked on the watch, I went directly to the clock, which was in another chamber, that I might see whether that agreed with the watch, and found that it was stopt at 4^h 11' 35". Its stopping, however, was not immediately owing to the violence of the shock, though several clocks, and watches too, at Boston, are said to have been stopped by it, but to the following accident: Having some time before used a pretty long glass tube, in a particular experiment, I had shut it up in the clock-case for security; and this tube, being overthrown by the earthquake, lodged against the pendulum, and stopt its motion. By this accident, the beginning of the earthquake, I conceive, is determined with all the exactness, that can be desired; for, so far as I can learn, the first shake was violent enough to overset so tall, slender a body, and standing in a position so near a perpendicular, as that tube; and it was impossible for the pendulum to make one oscillation, after the tube had struck against it. But
I am

I am not able to fix the end, nor consequently the duration, with the same exactness: however, from the time, when the clock stopt; to my looking on the watch, it was about 3' 1-half; and the jarring was not quite over till about a minute after this: so that I think I speak within bounds, if I say, that this shock with us lasted at least 4'. In other places, its duration might possibly be different. I was careful to note the time, when we had it, as exactly as I could, in hopes, that, by comparing it with the like accounts from distant places, we might be able to judge, with a good degree of exactness, of the course of this earthquake, the place of its origin, and the velocity of its progress. But all the accounts of the time, which I have yet seen, are so very lax, that no just conclusions can be drawn from them, with respect to either of these points. What I have been able to collect with relation to them, I shall set down presently.

Those, who suppose the duration much shorter, as 1' 1-half, or 2', urge, that a minute is a longer space of time than most people are aware of; which is very true: but it should also be considered, that if we judge of the length of any space of time by the number of ideas, which pass through our minds in that time; a very great fright,---a fright so great, as to take intire possession of the mind for a time, and shut out every idea except that of the present danger, will make us judge the time to be much shorter than it really is. And this, I make no doubt, was the case with many; the surprise, into which they were thrown, being such, as to keep out every idea, except that, which happened to strike their minds with the
greatest

greatest force at the beginning. Thus, several perceived no noise distinct from that, which was occasioned by the crackling of their houses, and the disturbance of the moveables in them; while others, who were waked by the noise, and ran from one room to another, have told me, that they felt nothing at all of the shake. For this reason, the conjectures, which persons in these circumstances made, as to the duration of the shock, ought not to be put into the balance with the actual observations made by watches.

The course of this earthquake seems to have been nearly from N.W. to S.E. My neighbour before-mentioned, who was then abroad, and informed me, that the noise began about the N.W. told me at the same time, that it passed off towards the S.E. and that he heard the noise in that quarter gradually abating, as it became more distant, for about the same space of time after the shock was over here, as he heard it in the N.W. before the shock began here. Other accounts, which I have since met with, agree with this. Those, who were in such clear open places, could make the best judgment in this matter; for such, as were within doors, or surrounded with buildings, might easily be misled by the various reflections of the sound. I am induced to give the greater credit to this information by what I observed myself: for a key, which was thrown from off a shelf in my house, was found at a place on the floor, which bore very near N.W. of the place, from which it fell; though the situation of it before its fall was such, that it might have been thrown in any direction, except towards the S.E.

An

An account, which we have lately received from the West-Indies, agrees very well with the supposition, that our earthquake proceeded south-eastward. The account is, that 'on the 18th of November, about two o'clock in the afternoon, the sea withdrew from the harbour of St. Martin's, leaving the vessels dry, and fish on the banks, where there used to be three or four fathom water; and continued out a considerable time; so that the people retired to the high land, fearing the consequence of its return; and when it came in, it arose six feet higher than usual, so as to overflow the low lands. There was no shock felt at the above time.' As this extraordinary motion of the sea happened about 9^h after our great shock, it seems very likely to have been occasioned by the same convulsion of the earth. Now if this earthquake went off south-eastward into the Atlantic, it must have passed considerably to the eastward of St. Martin's; and, in fact, it did not reach that island, there being no shock felt there. The motion of the sea then was owing to a great agitation raised at a considerable distance in some part or other of the ocean, where the earthquake passed, and from thence propagated to that island. Nor is the length of time greater than what seems to be necessary for this effect. The earthquake itself, at the rate it moved with us, would be some hours in going from hence to the distance of St. Martin's: for sound would be about $2\frac{1}{4}$ h in moving to such a distance; and the progress of the earthquake was slower than that of sound, as appears from hence, that the roar of this earthquake arrived here near a minute before the shake. The rest of
the

the 9^h might well be spent in conveying the motion excited in the water, from the place where it was excited, to St. Martin's; for the waves raised thereby could not move with near the velocity of sound.

It is worthy of remark, that, of the five great earthquakes, which this country has felt since its settlement by the English, two have gone nearly in the same track as this last did. The first, which was on June 2. 1638, 'came from the northward, and passed 'southward.' By the description given of it, it was very much like our late earthquake, only perhaps not quite so violent. 'The noise and shakes of the 'earthquake, October 29. 1727, seemed,' it is said, 'to come from the north-westward, and to go 'off south-easterly; and so the houses seemed to 'reel.' As to the great earthquakes of 1658 and 1662, we have no account of the courses, which they went in. But, from the 'other three, it may be reasonably conjectured, that the source of our earthquakes, or the place in which they originate, is in some part of Canada, or perhaps beyond it.

The extent of this earthquake seems to have been greater than that of any of our former earthquakes. This province of the Massachusetts-bay, or rather the province of New Hampshire, about the latitude of 43° north on the sea-coast, seems to have been the center of it, or the place of its greatest violence; and the shake to have been less considerable each way from hence towards the S. W. and N. E. By the accounts we have from the S. W. the shock was less at New York than it was with us; and still less at Philadelphia, which is farther towards the S. W. By the best information I can procure, the

limit toward the S. W. was Chesapeake-bay in Maryland, the shock having been felt on the eastern side of that bay, and not on the western. For the other limit toward the N. E. we are informed, that the earthquake was felt at Annapolis Royal in Nova Scotia, though in a much less degree than with us. It shook off a few bricks from the tops of some chimnies, but was not perceived by vessels on the water. And a letter from Halifax says, ' The earthquake, which ' happened in the W. extended itself to this place, ' tho' scarcely perceivable here.' But it was not at all felt by our army, which lay encamped at Seganeeto, about 100 miles N. from Halifax. Thus Halifax seems to have been very near the N. E. limit. I am not able to ascertain its eastern and western limits ; but it extended to all our back inland settlements ; and was perceived, though in a very small degree, by our army at Lake George, distant from hence about 130 miles N. W. by W. But it was not felt at all at the British fort of Oswego, situate on the south-eastern shore of Lake Ontario, and distant from hence about 250 miles W. by N. So great was the shock in the Atlantic, 70 leagues to the E. of Cape Anne, that the people on board a vessel there were suddenly surprised, just at the time of our earthquake, supposing they had run a-ground ; till, on throwing over the lead, they found they had more than 50 fathom water. The extent of the earthquake E. and W. from Halifax to Lake George was about 550 miles ; and its extent along the sea-coast, from N. E. to S. W. at least 800 miles. But if the agitation of the water at St. Martin's was occasioned by our earthquake continued into the Atlantic, as was conjectured
above,

above, its extent, in a direction toward the S. S. E. must have been at least 1900 miles.

I shall now proceed to mention the principal effects of this earthquake, for which I can find sufficient vouchers; for many strange things have been related, which, upon examination, appear to be without foundation. Besides the throwing down of glass, pewter, and other moveables in the houses, many chimnies were levelled with the roofs of the houses, and many more shattered, and thrown down in part. Some were broken off several feet below the top, and, by the suddenness and violence of the jerks, canted horizontally an inch or two over, so as to stand very dangerously. Some others were twisted, or turned round in part. The roofs of some houses were quite broken in by the fall of chimnies; and the gable ends of some brick buildings thrown down, and many more cracked. Throughout the whole country, the stone fences were more or less thrown down. The vane upon the public market-house in Boston was thrown down; the wooden spindle, which supported it, about five inches in diameter, and which had stood the most violent gusts of wind, being snapt off. A new vane, upon one of the churches in Boston, was bent at its spindle two or three points of the compass; and another at Springfield, distant about 80 miles westerly from Boston, was bent to a right angle. A distillers cistern, made of plank, almost new, and very strong put together, was burst to pieces by the agitation of the liquor in it; which was thrown out with such force, as to break down one whole side of the shed, that defended the cistern from the weather; as also to

flave off a board or two from a fence at the distance of eight or ten feet from it. In some parts of the country, particularly at Pembroke and Scituate, about 25 miles S. E. from hence, there were several chasms or openings made in the earth, from some of which water has issued, and many cart-loads of a fine whitish sort of sand. These are the principal effects of this earthquake on the land, some of which argue a very quick and violent motion of the earth. Tho' the degree of violence was doubtless different in different places, yet, that I might make some estimate of it with us, I measured the greatest distance on the ground, to which any of the bricks, which were thrown off from the tops of my chimnies, had reached, and found it to be 30 feet, and the height from which they fell was 32 feet. Now since bodies fall thro' 16 feet nearly in 1" of time; and the times, in which they fall through other heights, are in the subduplicate ratio of those heights; it follows, that the velocity, wherewith those bricks were thrown off, was that of above 21 feet in 1" of time: for the subduplicate ratio of 32 to 16 is the same as the simple ratio of 30 to a little more than 21. But the velocity was less at less heights: for the key before spoken of, as thrown from off a shelf in a chamber in my house, was not thrown so far, in proportion to the height thro' which it fell, as the bricks were from the top of the chimnies; and in my lower rooms nothing was thrown down, but a small bell in the garret was made to ring by it. Hence it appears, that our buildings were rocked with a kind of angular motion, like that of a cradle; the upper parts of them moving swifter, or thro' greater spaces in

in

in the same time, than the lower ; the natural consequence of an undulatory motion of the earth.

But the agitation occasioned by this earthquake was not confined to the land : it was very sensible on the water, and even at considerable distances in the ocean. The vessels in our harbours were so shaken, that it seemed to those, who were in them, as if they were beating on the bottom. Some, that were in the bay, coming in from sea, thought they had run upon rocks or sands. One very uncommon effect of this concussion is related by several of our seafaring men, that almost immediately after the earthquake, large numbers of fish of different sorts, both great and small, came up to the surface of the water, some dead, and others dying.

The center of our former earthquakes, as well as of this, seems to have been near the river Merrimac, about the latitude of 43° north, and 40 miles north from hence ; many shocks having been felt in that neighbourhood, which did not extend to this place. The late Rev. Mr. Plant of Newbury, which is situated at the mouth of that river, has given a very particular journal, in *Philos. Transact.* N^o. 462. of the shocks felt there from 1727 to 1741, few of which were perceived here or at Boston. I remember none after the memorable 29th October 1727, beside that on 30th January 1728, about two in the afternoon ; and that on 5th September 1732, which, by his account, did considerable damage at Montreal in Canada, but it was very small at Boston. That also on 6th February 1737, about a quarter past four in the afternoon, which he calls a considerable shock, was perceived at Boston ; and so was that on 7th December

December following, a little before 11 in the night. From the conclusion of Mr. Plant's journal, till the earthquake which is the subject of this letter, I know of none, but that which happened on Sunday June 3. 1744, at a quarter after ten in the morning. The roar of this was as loud as any I ever heard, but the shake not so great. The day was very fair and hot, with a little wind in the morning at W. S. W. which in the afternoon came round to N. N. W. The season preceding was hot and dry, there having been no rain from 23d May. On the 1st June, at four in the afternoon, Hauksbee's thermometer stood at 5,2; on the 2d, at five in the afternoon, it was at 1, with high wind at S. W.; on the 3d, at eight in the morning, it was at 19,8; and at six 1-half in the afternoon at 3,8. From the 1st June to the 2d, at the hours just mentioned, the barometer had fallen from 29,92 to 29,82; from which time it continued rising till the 4th at eight in the morning, when it was got up to 30,12; being, at the time of the earthquake, at 29,94. The rest of the month the weather was in general very hot, with many thunder-showers.

As the late Hon. Judge Dudley, who has given a very just account of the great earthquake of 29 Oct. 1727, in *Philos. Transf.* N^o. 437, has inserted an account of the weather in the preceding part of that year; and as our last earthquake happened at the same time of the year as that did, within 8 or 9 days (regard being had to the difference between the Julian and Gregorian styles); I hope it will not be disagreeable, if I give an account of our weather the last year: in doing which, I shall follow, as near as may be,

be, Mr. Dudley's method, setting down the particulars in corresponding columns, that so a comparison may more easily be made between these two years.

WEATHER in

1727, O. St.

1755, N. St.

January and February very moderate.

January, but especially February, very moderate.

Beginning of March, a great deal of snow, and some cold weather: afterwards, pleasant, rain at times, and once thunder and lightning.

4th of March the greatest storm of snow we had all winter. The whole month colder than February.

April, for the most part, fair, pleasant. Plentiful rain, beginning and end of the month.

April, nothing very remarkable. No hot weather. Each of these four months afforded more snow and rain, than the common quantity, taken at a medium for 7 years together.

May, beginning, pleasant; then a great deal of rain; afterwards, cold and very dry.

First 20 days of May, dry; 14 to 18 inclusive, uncommonly hot; latter part, frequent thunder-showers. The whole month drier than the medium.

June, abundance of thunder and lightning.

June, ten thunder showers; 15 to 20 inclusive, uncommonly cool. The driest June since 1749.

July,

WEATHER in

1727, O. St.

1755, N. St.

July, very dry; a great deal of thunder and lightning.

August, exceeding hot and dry. One plentiful rain.

September, till the middle, very hot. More hot weather than in any summer. Middle, a violent north-east storm, with a great deal of rain.

October, a pretty deal of cold weather.

23, a great deal of rain, with the S. wind.

25, at night a hard frost.

26, Winterish weather, and a little snow.

28, Cold. Wind N.W.

29, Cold. Little wind at N.W. Evening quite calm, and a clear sky.

days 3,404 inches of rain; and on the 11th in the morning there was thunder and lightning with the

July, seven thunder-showers, and a little more rain than the medium.

August, not very hot; much drier than the medium.

September, variable; 10 to 14 inclusive, uncommonly hot; several other days uncommonly cool. Upon the whole, the summer rather cool than hot. The hottest weather was in the middle of May. No great rains; but rather more than the medium.

October, a great deal of cold weather. Thunder-showers on the 13th and 17th. Snow on the 20th, 25th, 29th, & 30th. But the quantity of rain and snow in the whole month less than the medium.

November began with cold and wet, there falling, in the eleven first days 3,404 inches of rain; and on the 11th in the morning there was thunder and lightning with the rain;

rain; and at a quarter past two in the afternoon, the barometer was at 29,46; which was lower than it had been since the 15th of October. From thence, till the day of the earthquake, my diary stands t

November 1755.

D. H.	Barom.	Therm.	Han- k. b.	Wind.	Weather.	Rain, &c.
12 7 $\frac{1}{2}$ M	29,78	63,7		W 1	Very fair. Somewhat foggy.	,003
2 $\frac{3}{4}$ E	82	51,7		W 1	Fair.	
13 9 M	30,14	68		NW 2	Very fair.	
6 $\frac{1}{2}$ E	21	56,8		NNW 1	Clear.	
14 8 $\frac{1}{2}$ M	42	69,4		o	Fair.	
1 $\frac{1}{2}$ E	45	59		NE 2	Very cloudy.	
8 $\frac{3}{4}$ E	5	.		NE 1	Clear.	
15 8 $\frac{1}{2}$ M	4	74,6		o	Cloudy. Hazy. White frost.	
4 E	32	60,5		E 1	Very fair.	
16 0 $\frac{1}{4}$ M	27	70,8		o	Foggy.	
2 E	28	59,9		N 1	Fair.	,013
9 $\frac{1}{4}$ E	32	.		N 1	Fair with clouds. Foggy.	
17 8 M	3	70,1		N 1	Cover'd. Foggy.	
1 $\frac{1}{2}$ E	27	59		E 1	Very fair. Even ^s somewh ^t foggy	,001
18 4 $\frac{1}{4}$ M	17	74,1		o	Clear. A violent earthquake.	
8 M	16	78		o	Very fair. Great white frost.	
3 $\frac{1}{4}$ E	11	58,3		E 1	Very fair and hazy.	,017
10 E	08	69,1		o	Clear. Somewhat hazy.	

From this time the barometer rose till the 20th, when, at 8 $\frac{1}{4}$ M. it was up at 30,44, the sky covered, wind N 2. Then it fell till the 23d at 6 $\frac{1}{2}$ E, when it was so low as 28,87; which was lower than it had been since the 6th of February last. The afternoon of the 22d, and night following, when we had another shock, it was calm, and rained 1,205 inches. This leads me to observe, that though the *serenity*, as well as *calmness*, of the air, is a circumstance taken notice of in many earthquakes, both in this and in other parts of the world; yet it does

not always obtain, at least in the smaller shocks; and, so far as I have had opportunity to observe, the *calmness* of the air has more constantly attended upon earthquakes, than its *clearness*. The white frost on the morning of the earthquake, which, when melted, I found to be of the depth of $\frac{17}{1000}$ of an inch, was almost double of any white frost we have had for seven years past, and about five or six times as great as we commonly have. The barometer and thermometer underwent no alteration at the time of the earthquake: only, my barometer, which has an open cistern of quicksilver, and stood in a chamber, was so agitated, that part of the quicksilver was dashed over the sides of the cistern, and scattered upon the floor. This cistern was a cylindric cup, whose sides were an inch higher than the surface of the quicksilver.

I shall not pretend to make a comparison between the weather of the two fore-mentioned years, nor inquire how far Mr. Dudley's conjecture (*Phil. Trans.* N^o. 437. p. 66.), as to the influence of the weather in producing the earthquake of 1727, might be affected by such a comparison. I choose to leave this to you, Sir, and to the other gentlemen of the Royal Society, who, I know, are much better able to make a proper judgment in this matter; and beg leave to subscribe, with the greatest respect to that illustrious Society and yourself,

Reverend Sir,

Your most obedient,

and most humble Servant,

John Winthrop.

II. *The*

Cambridge in New
England, 10 Jan.
1756.

II. *The strange Effects of some effervescent Mixtures ; in a Letter from Dr. James Mounsey, Physician of the Russian Army, and F.R.S. to Mr. Henry Baker, F.R.S. Communicated by Mr. Baker.*

Moscow, Sept. 20th, 1756.

Read Jan. 20, 1757. **M**R. Butler, a paper-stainer, trying to make some discoveries for the better fixing of colours, was put in great danger of his life by the following experiments :

Having put into one gallipot a quarter of an ounce of verdegris, and into another pot two leaves of false-gold leaf, to each he poured about a spoonful of aqua-fortis. They began immediately to ferment, especially the gold-leaf. He was very assiduous in stirring them, to make the solution perfect. Having nothing else at hand, he did this with a pair of small scissars, at arm's length, carefully turning away his face, to prevent the fumes from entering his lungs. He was called away, about other business, before he had quite ended his process ; and soon after washed and shifted himself : but had scarce finished before he felt a burning pain in the ring-finger of his right hand, which he imputed to his having inadvertently touched the aqua-fortis. This increased every moment, and affected the whole hand with burning pain and swelling, which very soon subsided : but then it flew into the left hand, and, a few minutes afterwards, into the insides of his legs, as if scalding water had

been thrown on them. His stockings being immediately pulled off, there appeared a great many red spots, as large as six-pences, something raised above the skin, and all covered with very small blisters.

In about two hours after the accident, I first saw him : he was very uneasy, complaining of pain, and great anxiety, at the pit of the stomach, as if a burning hot iron was laid on it : so he expressed himself. His pulse was regular, but slower and weaker than natural : he had a nausea, and complained of a very coppery smell and taste. I ordered some alkaline volatile medicines, and to drink small sack-whey. He vomited once, and had four or five stools, and then his stomach grew easy. But the scene soon began again with lancing pain in the left eye. He continued the same medicines, drank plentifully of the whey, and was kept in a breathing sweat, by which he found some ease at night : but whenever the sweating lessened, the burning pains returned in broad flakes, changing from one part of the body to the other ; sometimes with shootings in his eye, and sometimes along the penis, but he had no heat of urine. His pulse continued regular, but weak ; and in several places of his body such kind of spots struck out as those on his legs.

Monday, the third day, in the morning, after sleeping well, his pulse was somewhat raised, and he continued easy till about eleven o'clock, when the burning pains returned, shooting from place to place ; but always so superficial, that he could not distinguish whether it was in or under the skin. Rubbing the part affected with one's hand gave ease : but when the sweating went off, and the burnings and shoot-
ings

ings became insufferable, I always put him into a bath of hot water, with some wood ashes, kept ready in the room; which gave him great relief. This afternoon he felt violent burning pain in his great toes, and sometimes in his left hand, with shootings up to the shoulder. Once he cried out, in great pain, that his shoulder was burst; for he felt something fly out with a sort of explosion: but, examining the part, I found nothing particular. He observed, when the flaky burnings began, they were as if they kindled from a point, and flashed like lightning, as he termed it. He was very often tormented with such pains on the pit of the stomach; and this evening had shootings thro' the back, with a pain in the belly. He complained of a strong sulphurous smell, which, he said, was like to suffocate him; tho' his breathing seemed easy, and his lungs no way affected. In the night he was seized with great pain about the heart, and cried out violently, that his heart was on fire: but after taking a dose of nervous medicines, and being put into the bath, he was soon freed from this, and passed the rest of the night tolerably well. At the time of such violent attacks the pulse continued regular, but still slower and softer than usual.

Tuesday. He complained most of his toes, and now and then burning pains in the forehead.

Wednesday. This whole day it continued most in the toes of the left foot; but in the evening the pain on the stomach returned, which lanced to the left side, with dartings inwardly. He became so uneasy and restless, that I was obliged to add some opium to the other medicines; which answered very well.

Thursday.

Thursday. The pains kept most in the toes of the left foot.

Friday. Nothing particular, except his feeling, with sharp pain, a spark (as he called it) fly out of his right cheek, in the same way, he said, as that, which burst on his shoulder, but much less. He perceived no pain in that part before this; nor any thing after, besides a soreness, which lasted for some days. Hitherto he had been kept in a continual sweat: his appetite was greater than his allowance; his digestion good; and his rest indifferent. From this time he was not attacked by any violent symptoms; and could be quiet, tho' he did not sweat.

On Sunday he began to get out of bed; but was often seized with glowing pains, suddenly affecting different parts of the body; which seldom continued an hour in one part, but shifted from place to place: these he was troubled with, in a less degree, even long after he went abroad.

By care and watchfulness the violence of the symptoms were kept under; and, by the use of antidotes for poisons of the nature of what he received this from, the disease was overcome, and the patient recovered his perfect health and strength.

III. *Extract of a Letter of J. Wall, M.D.
to the Rev. Dr. Lyttelton, Dean of Exe-
ter, and F.R.S. concerning the good Effects
of Malverne Waters in Worcestershire.*

S I R,

Worcester, Dec. 22. 1756.

Read Jan. 20, 1757. **T**HE Malverne Water much de-
serves encouragement, several very
extraordinary cures having been done by it lately. I
propose to make a collection of the principal, and
publish them, as an appendix to my little treatise.
Amongst other remarkable instances of their great
effects are the following, which have happened this
year. A poor woman, formerly a patient in our
infirmary for a fistulous ulcer in the hip, and another
in the groin, which penetrated the abdomen, has
received her cure there, tho' she was reduced to so
great a degree, as to be thought incurable, and sent
into the country on a milk-diet, &c. as the last
resource. The discharge from the fores was pro-
digiously great, and so offensive, that she could
hardly be borne in a room. The water took off the
ill smell almost instantly; the discharge soon lessened,
and grew thick and well-conditioned; her hectic sym-
ptoms went off in proportion; and, by continuing
the use of the water for five or six months, she is
cured.

A woman with a phagedenic ulcer in the cheek,
throat, and nose, from an ozæna in the hollow of
the cheek-bone, received great relief this year, in
five or six weeks time; the external ulcer, which
had

had almost destroyed the whole cheek, being healed in that time, and the other parts much amended. Her affairs would not permit her a longer continuance at the well ; but she continues the use of the water at home, and finds great relief from it there. I hope another season will complete the cure.

Mr. Parry, of Clent, had his skin cleared, and perfectly healed, in five weeks ; tho', when he came to the well, he was covered with an elephantiasis ; for which he had tried most of the purging waters, and sea-water, under the direction of Dr. Russell, without effect. So bad was he, that he could not move a limb but the skin cracked, and ouzed out a filthy sanies ; and he left the mark of his body every night in his bed. The waters have also had another very surprising effect on him : for they have been his Helicon, and converted him into a poet ; he having written a poem on the occasion, which he shewed to Lord Foley and Dr. Dalton.

I know a Lady, who, we had great reason to fear, had an internal cancer, who has lately received great advantage from the use of these waters, after other things had been tried unsuccessfully.

I could send many more instances ; but the compass of a letter will not admit of it : and I should be afraid of having tired you already, did I not know, that it must give you pleasure to hear of its extensive utility. I am, Sir,

Your most obliged

humble Servant,

J. Wall.

IV. *An Account of the Carlsbad Mineral Waters in Bohemia: In a Letter to the Right Honourable the Earl of Macclesfield, President of the R. S. by the Rev. Jeremiah Milles, D.D. F.R.S.*

My Lord,

Read Jan. 20,
1757.

MR. Watfon having favoured the Society with an Account of Dr. Sprengsfeld's treatise on the Carlsbad waters, I have taken the liberty to submit to your Lordship some observations on the same subject, which I made during my stay in that place; together with some specimens of different sorts of incrustations, which are formed by those waters.

Carlsbad is a small town, situated on the confines of Bohemia, at the distance of 14 German, or 28 French, leagues west of Prague. It is remarkable for its warm mineral springs, which are said to have been accidentally discovered, in the year 1370, by the Emperor Charles the IVth, as he was hunting; from whom they received their present name of Carlsbad, or Charles's bath. These waters soon growing into repute, occasioned the building of a small neat town, consisting chiefly of houses calculated for the accommodation of the company, who frequent this place in the summer time. There are two warm springs, which rise in the middle of the town, very near each other: and tho' they are supposed to be of the same

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quality,

quality, yet, as one is much warmer, it is thought likewise to be more efficacious than the other. The former of these, called the Brudel, rises very near the bed of the small river Tepel, which runs thro' the middle of the town and is sometimes overflowed by it. The water issues with great force from the bottom of this spring, rising in a considerable body to the height of six feet perpendicular; and would force itself much higher, if it were confined within a narrower compass. The spring is inclosed with a square wall, within which are fixed three wooden pipes, which convey the water from the bottom of the spring into a reservoir; which distributes it into a number of small troughs, communicating with the several bathing-houses, which are built on both sides of the river for the use of the patients. This spring is so impetuous, that they are obliged to pave and ramm the bed of the river, lest it should force itself up in the channel: and I observed one place on the river side, where it had burst thro' the rock; and they had been obliged to confine it, by fastening down a large stone on the orifice.

The water of this spring is so hot, that you cannot bear your hand in it; and the inhabitants make use of it for scalding their pigs and their poultry.

The water, when put into a glass, has a bluish cast, not unlike that of an opal: and tho' I could not discover, that in 24 hours it had deposited the least sediment, yet there was a thin whitish scum collected on the surface; and I observed the same in the baths, where it was much thicker; and was of the colour, and almost of the consistence, of a wafer.

It

It has a salt taste when first taken from the water, and is made use of by the inhabitants for cleaning of teeth and scouring silver: it is called Baden Flaum.

Tho' this water does not deposit any sediment, yet it is remarkable for the speedy and strong incrustation of all bodies, which are put into it. Little plaister figures are sold here, on purpose to verify the experiment; which, tho' perfectly white when put into the spring, are, in eight-and-forty hours, entirely covered with a yellow incrustation. The same effect is observed on the pipes and channels, thro' which the water is conveyed. If care were not taken to clean them four or five times a year, they would be intirely choaked up; and in some parts, where it has not been necessary to clean them so often, I have seen them covered with an incrustation two inches thick. In surrounding and covering these wooden pipes, they do not change the nature of the wood; but it is observable, that they add great hardness and solidity to it: so that it is affirmed a piece of deal will last a hundred years in this water. The head spring is cleared out once in 30 or 40 years, with a very great expence: at which time they are obliged to break off all the stony incrustation, which had been made by the water since the last cleaning; and if neglected would (as it has sometimes actually done) choak the passages, and oblige the spring to find vent in some other place. The incrustations formed by these waters are of different kinds: that, which is made in the troughs and pipes, thro' which the water is conveyed after it comes above ground, is of a light sandy nature, of a loose contexture, and a
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bright

bright yellow. It is used by the inhabitants as a gentle corrosive for eating off proud flesh. There is another of a darker colour, and a much harder nature, which is found at the very mouth of the spring, where it bursts out of the rock. There are other sorts taken out of the subterraneous cavities of the spring at the time it was cleaned. In what manner they are formed, is not so easy to determine; unless there were an opportunity of observing in what manner and direction they lie within the spring. They seem to be an alabastrine spar, and are beautifully marked with strait veins of different colours, which may be supposed to have received their tinge from the different colour of the spring-water at the time when this sediment, or rather scum, was formed upon it. They find pieces of this kind most beautifully variegated; and some of them large enough, by finearing, to make tables: these polish very well, and are not much inferior to jasper in appearance. It is a part of the manufacture of the place, to work this sort of stone into snuff-boxes, cane-heads, and sleeve-buttons.

There is likewise another sort of incrustation different from all these, which was found some years ago, in digging for the foundations of the new parish-church, which is about 300 yards distant from the Brudel spring. They found there the same kind of water; but it did not rise with so great force as in the other spring: and they discovered in the cavities large masses of a stony concretion, which were a sort of pisolithi, most of them in a globular, but some in an oval form, from the smallest size to the bigness of a nutmeg; the former sort lying in masses, the latter generally single and detached: they are perfectly

perfectly white, hard, and smooth, and appear to consist of a great number of lamellæ formed round a central nucleus. This sort of incrustation has been found in no other place; but there are some of a browner sort, and more irregular shapes, which are taken out of the Brudel.

The medicinal virtues of these waters have been treated of by German authors. They are esteemed to be particularly efficacious in removing obstructions, and in cases of the stone and gravel; of which the treatise lately produced to the Society contains many remarkable proofs. They are much frequented in these and in other cases; so that they have generally 200 persons in a season drinking the waters. The season begins in May, and ends in August. They drink them in the following method. They begin with a purge; and assist its operation with ten or twelve chocolate-cups of the water, taken within five minutes of each other. The day following they take the waters in the same quantity, and at the same intervals, keeping themselves all the time in a warm room; which, with the warmth of the waters, occasions a most plentiful perspiration. This is repeated for seven or eight days, increasing daily two or three cups of the water, till they come to drink 25 or 30 cups a day. The operation continues from eight of the clock in the morning till noon. Some bleed once in the middle of the course, others not at all. After they have finished this course of drinking, they bathe two days successively, continuing in the bath half an hour, or longer, as their strength permits them, or their case requires. This is the whole course; which is repeated two or three times,

times, or oftener, as they find necessary. The whole is concluded with a gentle purge, tho' the waters themselves are of a laxative nature.

There is another spring in the town of the same nature, but not so warm, as the Brudel: it is called the Mill-spring, and is only tepid. Those of a warm or weak constitution make use of this instead of the other, both for drinking and bathing.

There are likewise several chalybeat springs in the neighbourhood of Carlsbad; one at half a mile, and the other at two leagues distance from the town. Both of them seem to resemble the water of the Pohun spring at Spa; but are not near so strong. They do not use them medicinally on the spot; but they are brought to Carlsbad, and sold, in order to be drank with their wine. I am,

My Lord,

With the greatest respect,

Grosvenor-street,
Jan. 19th, 1757.

Your Lordship's

Most obedient humble Servant,

Jeremiah Milles.

V. An Essay towards ascertaining the specific Gravity of living Men. By Mr. John Robertson, F. R. S.

Read Jan. 27, 1757. **S**OME time last autumn I had occasion to draw up a few examples on the use of a table of the specific gravities and weights of

of some bodies. Among other things, that occurred then to me, I thought it might be useful to know the specific gravity of men. In order to make some experiments on this subject, I got a cistern made, of 78 inches in length, 30 inches wide, and 30 inches deep: it was constructed as near a parallelopiped as the workman could, to prevent tedious operations in computing the horizontal sections of the cistern by the surface of the water. I then endeavoured to find ten persons, such as I proposed to make the experiments withal; namely, two of six feet high, two of five feet ten inches, two of five feet eight inches, two of five feet six inches, and two of five feet four inches. One of each height I proposed should be a fat man, and the other a lean one; but I could not succeed in procuring such men; and, after waiting till near the middle of October, I was obliged to put up with such, as would submit themselves to the experiment at that season of the year. They were all labouring men, belonging to the ordinary of Portsmouth yard, and, except one or two of them, who were middling sized men, were for the most part very thin and slim made persons. I had also provided a sliding measure to take their heights, and scales to weigh them in. Every thing being prepared, each man stript himself in an adjoining room, and put on a pair of trowsers for decency's sake: his height was first taken, then his weight, and then he immersed (fortified with a large dram of brandy). A ruler, graduated to inches, and decimal parts of an inch, was fixed to one end of the cistern, and the height of the water noted before a man went in, and to what height it rose when he ducked himself
under

under its surface; and of these several observations is the following table composed.

N ^o .	Heights.		Wt.	Ht. Water before immerfed. Inches.	Ht. Water when immerfed. Inches.	Water raifed. Inches.	Solidity.	Weight Water. Pounds.
	Ft.	In.	Pds.					
1	6	02	161	19,30	21,20	1,90	2,573	160,8
2	5	10 $\frac{3}{8}$	147	19,25	21,16	1,91	2,586	161,6
3	5	9 $\frac{1}{2}$	156	19,21	21,06	1,85	2,505	156,6
4	5	6 $\frac{3}{4}$	140	19,17	21,21	2,04	2,763	172,6
5	5	5 $\frac{7}{8}$	158	19,13	21,21	2,08	2,817	176,0
6	5	5 $\frac{1}{2}$	158	19,09	21,26	2,17	2,939	183,7
7	5	4 $\frac{3}{8}$	140	19,05	21,06	2,01	2,722	170,1
8	5	3 $\frac{1}{8}$	132	19,01	20,86	1,85	2,505	156,6
9	5	4 $\frac{1}{8}$	121	18,97	20,76	1,79	2,424	151,5
10	5	3 $\frac{1}{4}$	146	18,93	20,66	1,73	2,343	146,4

In making of these experiments, I remarked some inconveniencies, which I did not at first advert to, and which, at that time, I could not prevent. I intended, that each man should have got gently into the water, immerfed himself all but his head, and so have staid until the motion of the water had ceased; then he was suddenly to have ducked his head under, and have continued so a few seconds of time, until I had noted the rise of the water; and, after his leaving the cistern, another was not to go in until the water was free from motion. Could these things have been done, as I had projected, I could have recommended the foregoing table as sufficiently complete: but I must observe, that beside the men's being of different sizes from what I had desired, they were in too much haste to be dismissed (with another dram

after dressing); so that the water was not quite still when they got into the cistern: neither could I persuade all of them to lay themselves down gently, much less to keep their heads under water so long a time as one second: so that, in most of the observations, the surface of the water was far from being quite so still, as to render the measures perfectly exact, I being obliged to catch them, as it were, by taking the mean height between the librations. Moreover, the great area of the cistern was no inconsiderable bar to the accuracy I expected. However, as I do not recollect experiments of this kind anywhere recorded, these, perhaps, may give some satisfaction to such persons, who may have the curiosity to desire some knowledge on this subject. Were I to make any more observations of this kind, I would chuse an upright paralleliped, not above 18 or 20 inches in the side of the square; into which the person should let himself down by steps nailed to the side: for in so small an area the motion of the water would sooner subside; neither would the librations be any thing near so large as on a smaller surface.

One of the reasons, that induced me to make these experiments, was a desire of knowing what quantity of fir or oak timber would be sufficient to keep a man afloat in river or sea water, thinking that most men were specifically heavier than river or common fresh water; but the contrary appears from these trials: for, excepting the first and last, every man was lighter than his equal bulk of fresh water, and much more so than his equal bulk of sea-water: consequently, could persons, who fall into water, have presence of mind enough to avoid the fright

usual on such accidents, many might be preserved from drowning; and a piece of wood, not larger than an oar, would buoy a man partly above water so long as he had spirits to keep his hold. Some things herein advanced will perhaps more readily appear from the following relation.

The Lords of the Admiralty have appointed, for the exercise of the scholars belonging to the royal academy at Portsmouth, a small yacht; wherein, during the summer months, those young gentlemen are taught the practice of working a vessel at sea, under the directions of one of the master-attendants, assisted by eight or ten seamen. The last time this yacht was out, which was about the beginning of last October, one of the scholars was ordered to heave the lead. The youth was about thirteen years old, small of his age, and far from being fat; as he was stepping on the gunnel, he fell over-board: the sea was rough, and the yacht had great way; so that he was presently at a considerable distance from the vessel. The skiff was immediately let down; but the painter not being fast, the rope run an end, and the skiff went adrift. One of the seamen jumped over-board, got into the boat, brought her alongside the vessel, took in another man, and then went after the youth, whom they recovered, after he had been in the water more than half an hour. The young gentleman, relating the affair, said, that as he could swim very little, and judging he should sink if he strove against the waves, he turned on his back, and committed himself to their mercy. He kept himself perfectly calm; and observed, when a wave was likely to break over him, to hold his
7
breath,

breath, and to spurt out the water forced into his mouth. His hat, which happened to be tied by a piece of string to one of his coat button-holes, he often held up with his hand, as a signal where he was. Just before the boat came up to him he began to be faint, his eyes became dim, and he thought himself on the verge of sinking. This youth, who, by his prudence, saved himself from drowning, must, at that time, have been specifically lighter than water.

VI. *An Instance of the Gut Ileum, cut thro' by a Knife, successfully treated by Mr. Peter Travers, Surgeon, at Lisbon. Communicated by John Huxham, M.D. F.R.S.*

Lisbon, August 3d, 1756.

Read Jan. 27,
1757.

ANtonia Josée da Costa, one of the King's messengers, was attacked by two men, and, after receiving two blows on the head, was stabbed with a knife in the right hypogastric region, about three fingers breadth above the os pubis; the external wound being larger, as the knife was drawn obliquely towards the navel, and might be an inch and half in length, the perforation thro' the peritonæum about three quarters of an inch; the intestine ileum hanging out about ten or twelve inches, and quite pierced thro', the wound in the gut being large enough to admit my fore finger. After clearing the grumous blood with warm

water and Hungary water, the uninterrupted future was made on both perforations; then dilating the common integuments of the belly, the intestine was reduced, leaving the ends of the two threads at the superficies of the wound; and the external incision was sewed up by the interrupted future, and common dressings of lint and bandage applied. A clyster was given him immediately after the above operation, of oil of olives, the yolk of an egg, and warm water.

4th. This day I found he had passed in the most excruciating pains, attended with continual vomitings: his fever very high, pulse full and irregular: he was bled ten ounces this morning, and the like quantity this evening. The clysters were continued thrice a day, with a decoction of wormwood and camomile instead of the warm water, and an anodyne mixture of mint-water, liquid laudanum, and sugar, to be taken occasionally; also three ounces of syrup of rhubarb, with an ounce of the fresh-drawn oil of sweet almonds, to be taken, a common spoonful, every two hours.

5th. The bleedings were continued twice this day, three ounces each time, and the clysters were administered as yesterday. His pulse and fever very high; he vomited some excrements; and towards night complained of a singultus.

6th. His bleedings and clysters were continued as before. Finding his singultus and vomiting so very troublesome, I ordered him Dr. Huxham's tincture of the bark; which was taken, a tea-spoonful, six times a day, in a little mint-water; which indeed greatly relieved him: his singultus and vomiting became less frequent.

7th. I found his skin moist, and pulse softened. I remained with him about an hour, and found a plentiful perspiration throughout the body; on which I omitted his bleedings: the clysters were continued; and towards night he had a proper discharge by stool, very foetid, and inspissated.

8th. I found, for the first time, he had slept last night, and seemed much in spirits: the symptomatic fever something lessened; and he had purged last night, and this day, eight times.

9th. He had five stools; his nausea much abated; and a gentle diaphoresis continued.

10th. The singultus ceased; his vomiting very little; his pulse low, accelerated, and thread-like in its stroke; his purging violent; and he greatly complained of a most acute pain of the wounded parts. A paper of the following absorbent powders was given him every three hours in rice-water. Crabs-eyes and red coral prepared, of each one drachm, crude opium two grains: these were made for three doses, and given as above.

11th. He slept well; less pain; pulse more equal; his diarrhæa much the same.

12th. The threads, with which I had made the future of the intestine, came out of themselves: the wound well-conditioned; fever very little; his diarrhæa rather increased. He sent for me in the evening, being much alarmed, as he thought some liquids he had taken to have passed thro' the wounded parts.

13th. Yesterday he complained of great pains in his belly: the discharge from his wound was laudable matter, and in good quantity.

14th.

14th. He rested well, and was seemingly well beyond expectation. His diarrhoea still continuing troublesome, he took the hartshorn decoction, with an addition of diascordium.

15th. I cut off the threads of the external wound, and continued dressings of digestive in the common method.

16th. He grew visibly better each day after ; and on Sept. 7th I discharged him from any further attendance, his wound being intirely healed over, and he is in all respects very well, free from pain, or any inconvenience from the wound. He was kept seven and twenty days on chicken-broth, and never admitted to use any solids during that time : afterwards he was indulged with young chickens, &c.

VII. *An Account of a Visitation of the leprous Persons in the Isle of Guadaloupe: In a Letter to Mons. Damonville, Counsellor and Assistant-Judge at Martinico, and in the Office of King's Physician at Guadaloupe. By John Andrew Peyssonel, M.D. F.R.S. Translated from the French.*

S I R,

Read Feb. 3, 1757. **I** Received the letter, which you honoured me with, and the order for visiting the persons afflicted with the leprosy. I was sensible of the misfortune of being ordered upon that commission :

commiffion: I fay misfortune; for fuch you will perhaps think it, when you have read this letter.

It is now about 25 or 30 years fince a very particular difeafe fhewed itfelf in many perfons in this ifland Grande Terre. Its beginning is imperceptible: there appear but a few livid-red fpoats upon the fkins of the white people, and of a yellowifh red upon the blacks. Thefe fpoats in the beginning are not accompanied with pain, or any other fymptom; but nothing can take them away. The difeafe increafes infenfibly, and continues feveral years in fhewing itfelf more and more. Thefe fpoats increafe, and extend indifferently over the fkin of the whole body. Sometimes they are a little prominent, but flat. When the difeafe makes a progrefs, the upper part of the nofe fwells, the noftrils are enlarged, the nofe becomes softened; tuberosities appear upon the cheek-bones; the eyebrows are inflated; the ears grow thick; the ends of the fingers, and even the feet and toes, fwell; the nails become fcaly; the joints of the feet and hands feparate and mortify: ulcers of a deep and of a dry nature are found in the palms of the hands and foles of the feet, which grow well, and return again. In fhort, when the difeafe is in its laft ftage, the patient becomes frightful, and falls to pieces. All thefe fymptoms come on by very flow degrees, one after another, and fometimes require many years to fhew themfelves: the patient is fenfible of no fharp pain; but feels a kind of numbnefs in his hands and feet. Thefe people perform their natural functions all the while, eating and drinking as ufual: and when even the mortification has taken off the fingers and toes, the only ill confe-

consequence, that attends, is the loss of those parts, that drop off by the mortification ; for the wound heals of itself, without any application : but when it comes to its last period, the poor sick persons are horribly deformed, and truly worthy of compassion.

This shocking disease is observed to have several other unhappy characters ; as, 1st, that it is hereditary, and that some families are more apt to be seized with it than others : 2^{dly}, that it is infectious, being communicated *per coitum*, and also caught by keeping company with those so diseased : 3^{dly}, that it is incurable, or at least that no remedy has yet been found to cure it. They have in vain tried mercurials, sudorifics, and every other regimen used in venereal complaints, under a notion, that this leprosy was the consequence of some venereal taint : but, instead of being of service, these methods rather served to destroy the patients ; for, far from lessening the disease, the antivenereal medicines unlocked the distemper, the most dreadful symptoms appeared, and all those so treated perished some years sooner than the others, who did not take these medicines.

A very just fear of being infected with this cruel distemper ; the difficulty of examining infected persons before the disease came to its state ; the length of time of its lying concealed, by the care of the patients to keep it secret ; the uncertainty of the symptoms, which distinguish it in the beginning ; produced an extraordinary dread in all the inhabitants of this island. They suspected one another, since virtue and merit had no shelter from this cruel scourge. They called this distemper the leprosy ; and consequently presented several memoirs to the generals and
intendants,

intendants, laying before them all these facts above-mentioned; their just apprehensions; the public good; the trouble, that this distrust caused in this colony; the complaints and hatred, that these accusations occasioned among them; the laws made formerly against such leprous persons, and their expulsion from civil society. They required a general visitation of all persons suspected of this distemper, that such, as were found infected, might be removed into particular hospitals, or into some separate places.

These memorials were sent to court, which, giving due attention to these just representations, issued orders for the required visitations in the most convenient manner, for the good of the public and of the state.

In the mean time, the post of physician-botanist became vacant in the island of Cayenne. The minister was pleased to name me for it; and altho' this island was much more fertile in philosophical discoveries than all the others, he thought proper to change my destination, and sent me to this isle Guadaloupe; and did not forget the article of the leprosy in my instructions.

When I arrived at Martinico in 1727, Monsieur Blondel de Juvencourt, then intendant of the French isles, communicated to me both the orders of the court, and all the memoirs, that related to this affair. A tax was then laid upon the Negroes of the inhabitants of the Grande Terre, to raise a necessary fund for this visitation, thus made at the expence of the colony; and Monf. le Mercier Beaufoleil was chosen treasurer of this fund.

Being arrived at Guadaloupe, the Count de Moyencourt, and Mons. Mefnier, ordinator and subdelegate to this intendance, communicated to me the orders of the general and intendant. I began then to inform myself of the necessary instructions for acquitting myself of this dangerous commission, the disagreeable consequences of which I easily foresaw. I had so often heard of these leprous spots, that I judged it necessary to know, whether what was said was true: for I could not comprehend, that a disease, which has so dreadful an end, and the symptoms then so terrible, should continue ten or fifteen years without any other appearance than these simple spots; which, in themselves, had nothing very bad. I demanded an inquest to be made, in order to satisfy myself of this fact: several surgeons, as practitioners, and several honest inhabitants, as observers, were accordingly called together, who all proved the same fact in this inquest; which you, Sir, may, and must, have seen in the register of the subdelegation of this island. I am, most sincerely,

S I R,

Your most humble and obedient Servant,

August 10. 1748.

Peyssonel.

RESULT of the VISITATION.

Ist, **N**ONE of the patients, whom we visited, had any fever; and they all declared, that they found no inconvenience nor pain; but, on the contrary, eat, drank, and slept well, performing every natural

natural function ; which was proved by their plumpness, which appeared even when the disease was most confirmed.

2. The disease began to shew itself in the Negroes by reddish spots, a little raised, upon the skin, being a dry kind of tetter, neither branny nor scabbed, and without any running, but of a livid-red, and very ill-conditioned. The Negroes sometimes bring these spots with them from their own country. The spots are constantly found upon every person troubled with this disease ; and are in greater numbers, in proportion as the disease grows more inveterate.

3. Among the whites the disease shews itself at the beginning by spots of a livid violet colour, without pain ; which are followed by little watery bladders, particularly upon the legs, which burst, and leave small ulcers with pale edges, and different in their natures from the common ulcers.

4. In proportion as the disease increased, the hands and feet grew larger, without any signs of inflammation ; since neither redness, nor pain, nor any oedematous appearance accompanied it ; but it was the very flesh, that increased in bulk. And this growth of the hands and feet was not attended with any sharp pain, but only a kind of numbness.

5. This bloated state of the hands and feet was succeeded by white deep ulcers under the skin, which became callous and insensible ; and which emitted only a clear ferous matter like water, and were but little painful. Afterwards the ends of the fingers became dry, the nails became scaly, and, I don't know how, they were eaten away ; the ends of the fingers dropt off ; then the joints separated

without pain, and the wounds cicatrized of themselves, without the least need of medicines. In the increase of the distemper hardnesses and lumps were formed in the flesh, the colour became tarnished, the nose swelled, and the nostrils grew wide: at last the nose softened like paste, the voice became hoarse, the eyes round and brilliant, the forehead covered with tetters and lumps, as well as the face; the eyebrows became very large, the countenance was horrible, the breath foetid, the lips swelled, large tubercles were formed under the tongue; the ears grew thick and red, and hung down; and, such was the insensibility of all the parts, that we run pins thro' the hands of several, without their feeling any thing of it. In short, we were assured, that these people perished by degrees, falling into a mortification; and the limbs dropt off of themselves, without any considerable pain, continuing still to perform well their natural functions.

6. These leprous people lived thus easy, if I may be allowed the expression, for several years, even fifteen or twenty; for the disease begins insensibly, and shews itself but very slowly.

7. Antivenereal remedies, which were ordered for almost every patient we saw, were of no service: if they sometimes palliated some symptoms, they very often hastened the progress of the disease: besides, we never found the parts of generation at all infected, nor any thing, that looked like the pox, about them.

8. Some of these people had indeed particular symptoms. In some the hair fell off; which was replaced by a finer kind: in others, worms were found.

found in their ulcers: want of sleep, or frightful dreams, afflicted some; while others quite lost their voice, or it became effeminate like that of eunuchs; and others, we found, stunk extremely.

9. Almost all of them, being desirous of concealing their disorders, endeavoured to deceive us, by alleging false excuses for the causes of their sores and ulcers: the greater part of them pretended, that the rats had eaten off their toes, and that burns had caused their ulcers. These were the figures, that every where presented to us.

10. We were confirmed in our opinions by experience, supported by verbal process, that this was the state of the diseased; that the distemper could neither be the pox, nor the effect of an inveterate one: that it had no symptom of that disease; but that it had every character of what the ancients called leprosy, elephantiasis, or such other names, as they were pleased to give it. So that we do not hesitate to pronounce, that those infected with this disease, as we have described it, ought to be treated as leprous persons, and subject to the ordinances, which his majesty was pleased to issue against such persons.

11. Again, we are well assured, from our observations, that the distemper is contagious, and hereditary; and yet the contagion is not so active, nor poisonous, as that of the plague, small-pox, nor even as the ring-worm, itch, scald, and other cutaneous disorders: for, if that were the case, the American colonies would be utterly destroyed; and these persons so infected, mixed as they are in every habitation, would have already infected all the Negroes, whom they come near.

12. We believe, that this contagion does not take place but by long frequenting the company of the infected, or by carnal knowlege. Besides, we have observed, that even such long frequenting, or cohabiting with them, are not always sufficient to communicate the disease; because we have seen women cohabit with their husbands, and husbands with their wives, in the distemper, while one is sound, and the other infected. We see families communicate and live with leprous persons, and yet never be infected; and thus, altho' experience, and the information of the sick, prove the contagion, we are of opinion, that there must be a particular disposition in people to receive the poison of the leprosy.

13. As to what regards the distemper's being hereditary, it is assuredly so. We have seen intire families infected; and almost every child of a leprous father or mother fall insensibly into the leprosy; and yet, in several other families, we have seen some children sound, and others tainted; the father has died of the disease, and the children grew old without any infection: so that, tho' it is certainly hereditary, yet we believe it is of the same nature with those in families troubled with the consumption, gravel, and other hereditary distempers; which are transmitted from father to son, without being so very regular, as to affect every one of the family.

14. We could never find out any certain rule of judging, at what age the disease shews itself first in those, who were begotten by infected parents: but we have, as far as we could, observed, with regard to women or girls, that the symptoms begin with the menses, and continue slightly till they have lain-
in

in of one or two children : but that then more visible, and indeed more cruel, symptoms appeared. As to men, or infants, there is no rule to know it in them.

15. For the explanation of the causes, symptoms, and what we think the most likely means of cure, we refer to a particular dissertation. Let it suffice here to observe, that we do not imagine, that the air, water, or manner of living, can produce it; for we have found as many sick in the low marshy places, as in more airy saline places : and if many Negroes were infected in the Grand Terre, where they drink the foul waters of ponds and lakes, we see an equal number ill in places, where they have fresh rivers and running waters; but they may prove proper causes for unlocking, and disposing persons to receive, the disease.

16. We believe, and are persuaded, that the origin of this disease among the Negroes comes from Guinea : for almost all the Negroes from that country told us they came from thence with these reddish spots, the first and certain signs of the distemper begun.

17. As to the infected Whites and Mulattoes of this island, we were informed, that the disease was not known among the Whites till about 25 or 30 years ago; when, out of charity, they received a miserable object from the island of St. Christopher's, whose name was Clement; who, about the year 1694, fled hither. It was the family of the Josselins, called the Chaloupers, that protected him; which family, as also that of the Poullins, we found infected by communication with this sick man, as old Poulin declared to us.

It is thought, that others were infected by communication with the Negro women, especially in the beginning, when the disease is much concealed, and at a time when they did not mistrust one another; which is very probable, since we saw many Mulatto children, born of female Negroes, infected and leprous.

18. However this be, this distemper has had its progress; and in this visitation, which we made, we examined 256 suspected persons; that is, 89 Whites, 47 free Mulattoes, and 120 Negroes: among whom we found 22 Whites, 6 Mulattoes, and 97 Negroes, infected with the leprosy, amounting to 125. There were six Whites and five Negroes more, whom we could not visit, for reasons set forth in the verbal process. The remaining persons, which were 131, appeared to us very sound: not that we can answer for the consequences, especially with respect to the children, who are the offspring of leprous persons; whether declared such by us, or dead before the visitation, suspected of infection.

This is the opinion, declaration, and result of the visitation made by us, the physicians and surgeon appointed for that purpose. At Basseterre, the day above-mentioned.

PEYSSONEL.
LEMOINE.
MOULON.

A second visitation was made in October 1748.

VIII. *An Account of the late Discoveries of Antiquities at Herculaneum; in an Extract of a Letter from Camillo Paderni, Keeper of the Herculanean Museum, and F. R. S. to Thomas Hollis, Esq; dated Naples, Dec. 16, 1756.*

Read Feb. 10, 1757. **I**T is probable, that the first volume of antique paintings will be published at Easter; in which there will be fifty copper-plates, with observations by the academy lately established here for illustrating the antiquities.

Two volumes of the ancient papyrus have been unrolled. One treats of *rhétoric*, and the other is upon *music*; and both are written by the same author, Philodemus. Il Signor Canonico Mazzocchi, a very learned gentleman of this city, is now translating them from the Greek. There are two persons constantly employed in unrolling other volumes.

In the month of April were found two fine busts of women, the subjects unknown. Also a young stag, of excellent workmanship, upon a base. The height of it, from the feet to the top of the head, is three palms and a half. Likewise its companion; but broken in many pieces; which however I hope to restore.

In May, a small young hog.

In October, a female statue, of middling workmanship. Also a Silenus, a palm and three inches high, standing upon a square base raised upon three

rows of steps, which are supported at the angles by lions claws. He has a bald head, a long curled beard, a hairy body, and naked feet. The drapery about him is loose and flowing: the fore-finger of each hand is extended, and all the rest are closed. From his back arises a branch above the head, where it divides into two, which, twisting their foliage round it, fall and spread themselves below the shoulders, on each of which a stand is placed to fix a lamp. In the middle, betwixt the extremities of these two small branches, is a bird resembling a paroquet. The whole of this figure is in a very good taste. All these things above-mentioned are of bronze.

In November was discovered a beautiful marble *Terminus*, of Greek workmanship, as big as the life. It is dressed in a *chlamys*; has a young countenance; and the head is covered with a Grecian helmet.

Many other things have also been found, as lamps, vases, and such-like, in bronze. And we have often met with paintings. If any farther discoveries are made, which are remarkable, you may depend on being informed of them.

At present my time is much taken up, in a work extremely difficult and tedious; which is this: When the theatre was first discovered, there were found in it, among other things, several horses in bronze, larger than the life; but all of them bruised, and broken into many pieces. From this sad condition they are not yet restored. But his majesty having expressed a particular desire to see that effected, if possible, with regard to one of them, I resolved to attempt it; and accordingly have set about it.

IX. *An Account of some Trees discovered under-ground on the Shore at Mount's-Bay in Cornwall: In a Letter from the Rev. Mr. William Borlase, F. R. S. to the Rev. Dr. Lyttelton, Dean of Exeter.*

Reverend Sir, Ludgvan, Jan 24. 1757.

Read Feb. 10, 1757. **B**EING an airing the other day with Mrs. Borlase, on the sands below my house, we perceived the sands betwixt the Mount and Penzance much washed into pits, and bare stony areas, like a broken causey. In one of the latter, Mrs. B. as we passed by, thought she saw the appearance of a tree; and, upon a review, I found it to be the roots of a tree, branching off from the trunk in all directions. We made as much haste down to the same place in the afternoon as we could, and with proper help to make a farther examination. I measured and drew the remains; and about 30 feet to the west found the roots of another tree, but without any trunk, tho' displayed in the same horizontal manner as the first. Fifty feet farther to the north we found the body of an oak, three feet in diameter, reclining to the east. We dug about it, and traced it six feet deep under the surface; but its roots were still deeper than we could pursue them. Within a few feet distance was the body of a willow, one foot and a half in diameter, with the bark on; and one piece of a large hazel-branch, with its bark

on. What the two first trees were, it was not easy to distinguish, there being not a sufficiency remaining of the first, and nothing but roots of the second, both pierced with the teredo, or augur-worm. Round these trees was sand, about ten inches deep, and then the natural earth, in which these trees had formerly flourished. It was a black marsh-earth, in which the leaves of the juncus were intirely preserved from putrefaction. These trees were 300 yards below full-sea-mark; and, when the tide is in, have at least 12 feet of water above them: and doubtless there are the remains of other trees farther towards the south, which the sea perpetually covers, and have more than 30 feet water above them. But these are sufficient to confirm the ancient tradition of these parts, that St. Michael's mount, now half a mile inclosed with the sea, when the tide is in, stood formerly in a wood. That the wood consisted of oak, very large, hazel and willow trees, is beyond dispute. That there has been a subsidence of the sea-shores hereabouts, is hinted in my letter to you, p. 92; and the different levels and tendencies, which we observed in the positions of the trees we found, afford us some material inferences, as to the degree and inequalities of such subsidences in general; as the age, in which this subsidence happened (near 1000 years since at least) may convince us, that when earthquakes happen, it is well for the country, that they are attended with subsidences; for then the ground settles, and the inflammable matter, which occasioned the earthquake, has no longer room to spread, unite, and recruit its forces, so as to create frequent and subsequent earthquakes: whereas, where
there

there are earthquakes without proportionable subsidences, there the caverns and ducts under-ground remaining open and unchoaked, the same cause, which occasioned the first, has room to revive and renew its struggles, and to repeat its desolations or terrors; which is most probably the case of Lisbon. I am, Sir,

Your most affectionate

and obliged humble Servant,

Wm. Borlase.

X. *Experiments on applying the Rev. Dr. Hales's Method of distilling Salt-water to the Steam-Engine.* By Keane Fitzgerald, Esq; F. R. S.

Read Feb 17.
1757.

ON reading Dr. Hales's account of purifying salt water, by blowing showers of air thro', it occurred to me, that something of the kind might be applied with advantage to the steam or fire-engine, by increasing the quantity of steam, and consequently diminishing the quantity of fuel otherwise necessary.

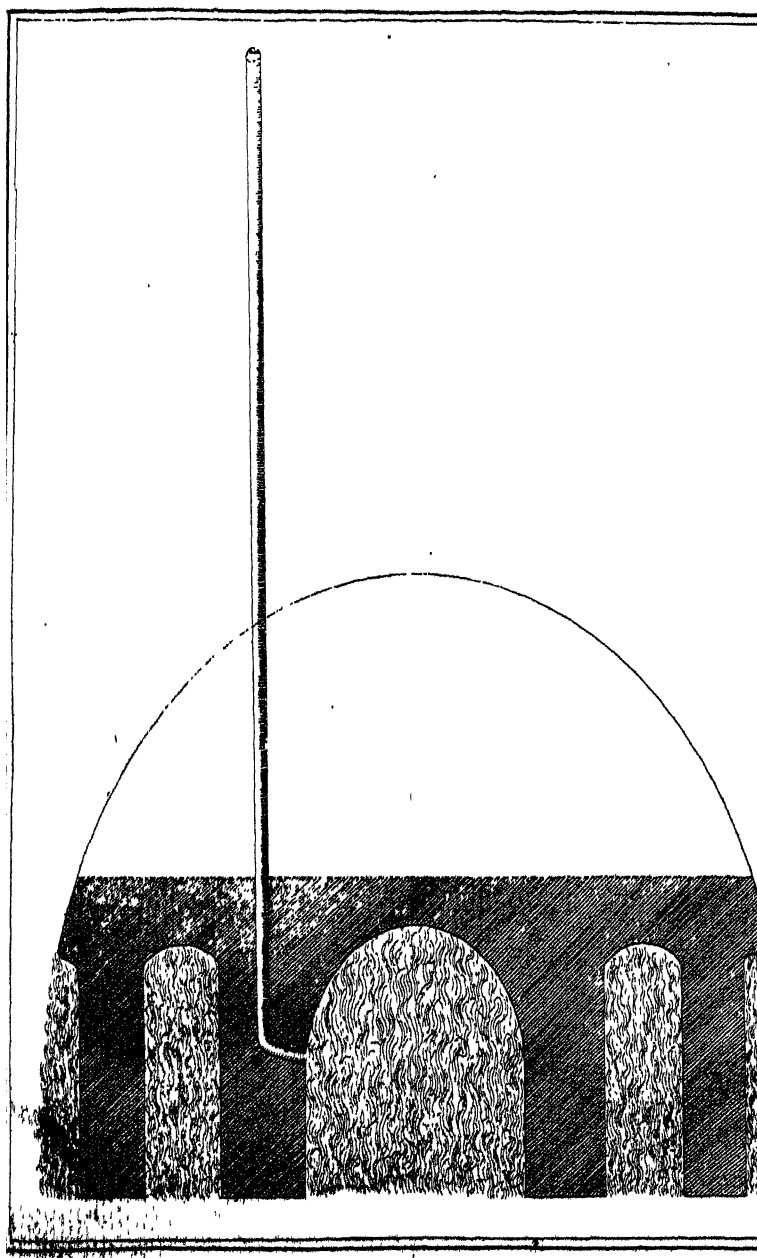
As the strength of steam raised from boiling water is always in a fluctuating state, and, by the best experiments hitherto made, has never been found above $\frac{1}{10}$ stronger, or weaker, than air; I was in doubt, whether steam, produced by this method, would

would be sufficiently strong for the purpose of the steam-engine.

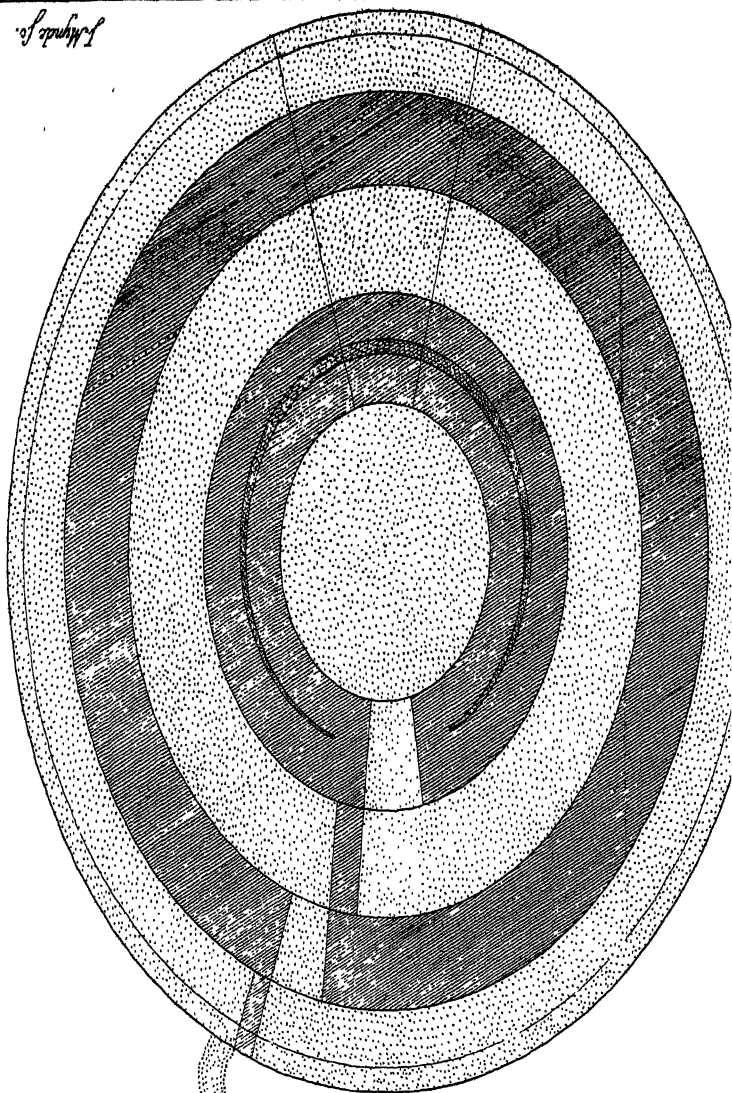
I made an experiment first on a small boiler, about 12 inches diameter, made in the shape of those commonly used in steam-engines, with a funnel at the top, of about 1 inch diameter, for the steam to pass thro'; the aperture of which was covered with a thin plate, fixed at one end with a hinge, and a small leaden weight to slide on the other, in the nature of a steel-yard, to mark the strength or quantity of the steam. A tin pipe made for this purpose, with several small holes towards the end, passed from a small pair of bellows, through the upper part of the boiler, to within about an inch of the bottom. The boiler was half filled with water, which covered the holes in the pipe about six inches. From the best observation I was capable of making with this machine, by blowing air thro' the boiling water, it produced about $\frac{1}{2}$ more steam than was produced by the same fire without blowing air thro'.

I then applied a machine of this kind to the engine at the York-buildings water-works, the boiler of which is 15 feet diameter. This is a patent-boiler, a section and plan of which is annexed. It has a double concave, with a kind of door-way or passage from one to the other, in order to let the flame pass, as it were, thro' and round the water; by which means there is no-where above nine inches of water to be heated thro', tho' the boiler is so large; and which, by three years experience, has been found to require $\frac{1}{4}$ less fuel, than any other fire-engine of equal bigness.

I fixt



Stylus. Sc.



Philos. Trans. Vol. I. TAB. I. p. 54.

I fixt a pipe of an inch and a half diameter to a pair of double bellows three feet diameter ; which pipe reached about one foot under the surface of the water in the boiler, to the end of which are fixed horizontally two branches, each about eight feet long, tapering from one inch diameter to about $\frac{1}{4}$ of an inch. These branches are bent in a circular manner, as in the plan, to answer the form of the concave, and are perforated with small holes about four inches distant at the thickest part, and decreasing gradually in distance, to within $\frac{1}{4}$ of an inch, towards the small end. The reason of these branches being made taper, and the distance between the holes decreasing to the small end, was in order to give the greater power to the air forced by the bellows to discharge the water lodged in such a length of pipe ; and I observed by this method, that the water was gradually forced thro' the holes to the end of each branch, and seemed to throw an equal quantity of air thro' the water.

The length of the pipe, to which the branches are fixed horizontally, is about 18 feet to the nose of the bellows : notwithstanding which length, the steam, that passed thro' the pipe into the bellows, was so hot before the water boiled, as to force thro' the leather : but this I easily remedied, by fixing a brass cock of one inch and a half diameter to the pipe, which hindered the steam from ascending, until the engine was ready to work ; and being opened, the air continually keeps it cold until the engine has done working ; then the cock must be shut again.

The bellows is worked by means of a small lever, and pullies applied to the great lever of the fire-engine,

engine, which keeps a continual blast whilst the engine works ; the strength of which is increased or diminished, by adding or taking off the weights on the bellows.

The effect produced, according to the best observations I could make, was, first, a very visible alteration for the better in the working of the engine. When the fire was stirred, as it must be every time fuel is added, the steam generally became too fierce, which occasioned great irregularity, and sometimes, if not watched, great damage to the engine ; and when the fire abated, the stroke became immediately much shorter, or stopped intirely, if fuel was not soon added : whereas, by blowing air thus thro' the water, it keeps, with any moderate care, an equal stroke to its full length, from the beginning to the end ; and by that means discharges a considerably greater quantity of water. A proof of which was very evident, tho' I could not ascertain the exact quantity : for the engine, before this improvement, supplied but two main pipes at once, which conveyed the water to the houses served by them ; but since could not take off the quantity of water thrown up, part of which was obliged to be discharged into a third main.

As to the quantity of fuel, that may be saved by this method, it is not easy to determine from any experiment on this engine, the boiler and fire-place of which is made very different from all others, and the quantity of fuel already thereby greatly lessened. The fire-place, which may be said to be within the boiler, and is but barely large enough to contain a quantity of the roundest and strongest burning coals
icient

sufficient to work the engine, cannot in this be made less; and consequently will not admit such a saving from this model, as from one properly constructed for the purpose: a proof of which I made, by trying some coals of a weaker kind, which were also cheaper; but on trial were not strong enough to work the engine, and had therefore been laid by. These coals answered extremely well; and, as it was a slower-burning coal, I found the consumption, whilst they lasted, was between two and three bushels less in every six hours, which is about the time the engine works each day: and I am satisfied, if the person, who attends the engine, would take the proper care, more coals could still be saved. For at several different times, when I had the coals exactly measured, and marked the time, I constantly found, that it required half a bushel in the hour less than he generally used, and the engine threw up as much water.

As this method of blowing air thro' boiling water, in order to increase the quantity of steam for a fire-engine, has, I believe, never before been attempted, and produces already a very good effect, I am in hopes it may be still further improved.

XI. *Extract of a Letter of Mr. Abraham Trembley, F. R. S. to Tho. Birch, D. D. Secret. R. S. Translated from the French.*

S I R,

Hague, 1 Feb. 1757.

Read Feb. 17, 1757. **I** Wrote to you on the 26th of November last, concerning the earthquake felt some time before between the Rhine and the Meuse. I have been since informed by Professor Donati of Turin, that a slight shock had been perceived there on the 13th of August 1756, at a quarter after nine in the morning. It was likewise felt in other parts of Piedmont. He has also communicated to me an extract of a letter of a professor of Genoa, one of his friends, of which the following is a translation.

“ On the 9th of November we felt here two
“ shocks of an earthquake; one at 20 $\frac{3}{4}$ hours, accord-
“ ing to the Italian way of reckoning; the other at
“ about 4 $\frac{1}{2}$ hours at night. I did not perceive the
“ first, being then walking in the house; but I felt
“ the second. I was then laid down, and going to
“ sleep. The direction of the undulations was from
“ north to south, as far as I could judge.”

Monf. Donati took last summer, according to his custom, a journey, in order to prosecute his researches into natural history. He was accompanied by Dr. Ascanius, Fellow of the Royal Society; who was still in doubt about coral's being a composition of animals. Monf. Donati carried him to the sea of Provence. He ordered coral to be fished up in his
7 . presence.

presence. He placed it in a large vessel full of water ; and carried this vessel on shore ; where he soon convinced Dr. Ascanius, by his own eyes, that coral is a mass of animals of the polype-kind.

Monf. Donati has written to me, that he has thoroughly satisfied himself by his last observations, that the polypes are fixed to their cells ; of which he had before doubted. What he says afterwards of coral appears to me to express with more truth and precision what we ought to think of this kind of animals, than any of the descriptions, which have been given since the new discoveries have changed our sentiments on that subject. Polype-beds, and the cells, which they contain, are commonly spoken of as being the work of polypes. They are compared to the honeycomb made by bees. It is more exact to say, that coral, and other coralline bodies, have the same relation to the polypes united to them, that there is between the shell of a snail and the snail itself, or between the bones of an animal, and the animal itself. Monf. Donati's words are as follow.

“ I am now of opinion, that coral is nothing else
 “ than a real animal, which has a very great number
 “ of heads. I consider the polypes of coral only as
 “ the heads of the animal. This animal has a bone
 “ ramified in the shape of a shrub. This bone is
 “ covered with a kind of flesh, which is the flesh
 “ of the animal. My observations have discovered
 “ to me several analogies between the animals of
 “ kinds approaching to this. There are, for instance,
 “ *keratophyta*, which do not differ from coral, ex-
 “ cept in the bone or part, that forms the prop of the
 “ animal. In the coral it is testaceous, and in the
 “ *keratophyta* it is horny.”

The observations, which I have made upon some kinds of polype-beds, lead me to think, that what are called polypes, in those bodies, which are observed to come out of and return into the cells, are more than the heads of the animal. I have seen some, which had a bag, into which pass'd their food, which I saw them swallow; and another bag, into which passed the grossest part of that food, after it was digested. This is the case, for instance, of the plumed polypes, which I described at the end of the third memoir, in the work published by me on one kind of fresh-water polypes.

Monf. Donati has observed divers very curious facts in the journey, which he made into the mountains. He has, in particular, traced out an immense bed of marine bodies. This bed crosses the highest mountains, which separate Provence from Piedmont, and loses itself in the plains of Piedmont.

He has likewise observed a mass of rock, which forms the extremity of a pretty high mountain, the foot of which is washed by the sea. This rock is, at a considerable height, intirely pierced by pholades, that species of marine shell-fish so well known, which digs cells in the stones. It appears from hence, that this rock was some time covered by the sea. According to Monf. Donati, the sea has insensibly retired from the parts, which were washed by it; and he thinks, that there must have been a very considerable space of time between that and the time, when this mountain, pierced by pholades, was covered by the waters of the sea. He deduces his opinion from the following fact. There is in this rock, pretty near the surface of the sea, a natural cavern

cavern fill'd with earth. In this earth have been found ancient Roman sarcophagi and lamps. It follows from hence, that even in the time of the Romans this part of the rock, in which this cavern is situated, was not under water. As there is but a small distance between the cavern and the surface of the water, it follows, that the water has sunk but very little since the time of the Romans. If it has sunk in the same proportion since the time, when it covered the top of the rock, there is no doubt, but that the time, when it was intirely covered by the sea, must have been very distant. If the same manner of reasoning be used, with respect to the bed of marine bodies, mentioned above, which crosses the mountains, that separate Provence from Piedmont, we shall be obliged to presume, that the time, when those mountains were under the waters of the sea, was at a very great distance from the present.

Monf. Donati concludes from these facts, and the consequences deduced from them, that the Mediterranean sea is a very ancient, and not a modern one, as Monf. de Buffon imagines.

Those, who explain all the phænomena of marine bodies found out of the sea, by an universal deluge, do not admit the consequences drawn by Monf. Donati from those marine bodies now under consideration. It is plain, that most of the naturalists, who have observed a great number of these marine bodies, are not of opinion, that all those phænomena can be explained by an universal deluge. Upon these subjects, before we undertake to judge, it is proper to be well informed of the nature of marine fossil bodies, which are found in divers parts, and of their situation and arrangement. It is necessary likewise to be acquainted

quainted with the state of those, which are found actually under the sea, and the revolutions, to which they are subject, while they are covered by it. It is still farther requisite to have an attention to the revolutions, which have been and are constantly observed, with respect to the sea-shores, which change their situation in several parts, some advancing upon the land, and others retiring. If all these different facts be compared together, it will not be doubted, but there are actually under the earth marine bodies, which are found there only in consequence of these slow revolutions, and not of an universal deluge. Perhaps this notion might be extended to the greatest part of the marine fossile bodies, which are known to us.

Monf. Donati informs me, that he would be glad to present to the Royal Society an history of coral, if he thought, that it would be agreeable to them.

XII. *A brief Botanical and Medical History of the Solanum Lethale, Bella-donna, or Deadly Nightshade, by Mr. Richard Pultney. Communicated by Mr. William Watfon, F. R. S.*

Read Feb. 17.

1757.

BELLA-DONNA is the name, which the Italians, and particularly the Venetians, apply to this plant; and Mr. Ray (1) observes, that it is so called because the Italian ladies

(1) Nom. Etymol. ad Calcem. Cat. Cant. p. 43. item Hist. Plant. p. 680.

make a cosmetic from the juice, or distilled water, which they use to make their complexion fair and white. Others (2) suppose it derives its name from its intoxicating quality. With us it is generally known by the name of Deadly Nightshade, or Dwale, tho' this last term is seldom used for it; and the old French word *Morelle*, which Lobel applies to it, seems to be quite forgotten amongst us.

CLASSICAL DISTRIBUTION.

The Deadly Nightshade was very soon discovered by the revivers of botany after the restoration of learning; and, agreeable to the fashion of those days, it was greatly debated among the commentators, whether it was known, and by what name, to the fathers of botany Theophrastus and Dioscorides. Several of the writers of that time, as Dodonæus, Guilandinus, Fuchsius, and Cordus, were of opinion, that it was the *Mandragora morion* of Theophrastus; and their sentiments were espoused by his learned commentator Bodæus à Stapel (3), who moreover supposes it the plant, which Dioscorides describes, lib. iv. cap. 69. under the name of *Στρυγχνος μαρινός*. On the other hand, Matthioli (4) has taken great pains to prove, that it is not the *Mandragora* of Theophrastus; and both he and Ruellius (5) are

(2) *Bella-donna dicitur quod imaginationes non injucundas efficiat, vel ut honeste satis Plinius, quod lufum generet.* Bod. Comment. in Theophr. p. 586. quod in somnis pulchras ostendat virgines feminasque. Ibid. p. 1078.

(3) *Locis citatis.*

(4) *Oper. omnia edit C. B. p. 756.*

(5) *Ruell. in Dioscor. p. 536.*

inclined to think, that the *Bella-donna* was not known to either of the Grecian Fathers; who are so short, vague, and immethodical, in their descriptions, that it is very difficult, not to say impossible, to apply them to particular species with justness and precision.

Be this as it will, our restorers of botany agreed in general to rank it with the Solana, or Nightshades; and as most of them took it to be the *Στεφύλιος μαυικός* of Dioscorides, so we find thereto the addition of some epithet, expressive of its deleterious quality, in most of their writings; such as *lethale*, *somniferum*, *furiosum*, &c. Its general agreement with the plants of that genus, and also the knowlege the world soon had of its poisonous quality, when it is considered, that systematic distributions, from the parts of fructification, had not been thought of at that time: these, I say, were sufficient reasons for referring it to the Nightshades. By such names therefore is it found in most of the old writers; till Clusius, who, observing perhaps, that it differed in its parts of fructification from the Solana, adopted the indigenous Italian name, as a generical one, and called it *Bella-donna*. Cæsalpinus, the first inventor of a botanic system, did not separate it from the Nightshades. Morison and Ray, the revivers of method almost an hundred years afterwards, were aware of the difference; the former having placed it in a chapter among the *Solanis affines*, and the latter constituted a distinct genus of it, tho' he retained the old name in his history of plants. Tournefort adopted Clusius's name *Bella-donna*, and was followed by all the systematic botanists, who have since wrote; as Boerhaave, Rivini, Ruppius,
Knaut,

Knaut, Magnol, Ludwig, and Haller; until Linnaeus, conformable to the 229th rule of the *Fundamenta Botanica* (6), rejected it, and very expressly calls it *Atropa* (7); in which he is followed by all succeeding writers, who have chosen his method.

Cæsalpinus, Morison, Ray, Herman, and Boerhaave, who range these plants according to the fruit, place the Deadly Nightshade among the *Herbæ Bacciferae* in their respective systems.

Rivinus, Ludwig, and Christian Knaut, who adopt the number and regularity of the petals in the corolla, for their classical character, refer it to such as have regular monopetalous flowers. Ruppius, whose method is upon the same plan, brings it among the irregular monopetalous ones.

Tournefort's method, which is established upon the figure of the flower, takes it into the first class among such plants as have campaniform or bell-shaped flowers.

Dr. Van Royen, whose system is undoubtedly a very elegant attempt towards the natural method in botany, arranges it among such as he calls *Oligantheræ*; namely, such plants as have the stamina equal to, or fewer in number than, the segments of the corolla.

Dr. Haller, whose method is upon the plan of a natural one also, includes the *Bella-donna* among the *Isoetemes*, such plants as have the number of the stamina equal to the segments of the corolla.

(6) Nomina generica quæ ex Græca vel Latina lingua radicem non habent rejicienda sunt.

(7) *Atropos una furiarum.* Crit. Botan. p. 75.

In the sexual system of Linnæus, at this time so generally received, and so well established, it belongs to the Pentandria monogynia, or such plants as have five stamina and one style. The plants of this order are arranged into five subdivisions. The *Atropa* comes in among those, that have declinated stamina. According to this method, we shall give its generical characters from the last edition of Linnæus's *Genera Plantarum*.

The most obvious and essential character of the genus is the *globose berry, and open calyx* (8). The general character is as follows.

ATROPA Linn. Gen. Plant. Ed. 5. N°. 222.

The calyx is a gibbous permanent perianthium, formed of a single leaf divided into five acute segments.

The corolla is formed of a single bell-shaped petal, the tube of which is very short; the limb ventricose, of an oval figure, and longer than the calyx. The mouth is small, expanded, and divided into five pretty equal segments.

The stamina are five subulated filaments proceeding from the base of the flower, and are of the same length: at the base they are connivent, and at the top bent outwardly. The antheræ are thick and affurgent.

The germen is of a semiovated figure: the style is filiform, of the length of the stamina, and inclined. The stigma is capitated, transversely oblong, and affurgent.

(8) See Lin. Syft. Naturæ, edit. Lugd. Bat. 1756. p. 97. N°. 222.

The fruit is a globose berry, standing in a large cup, and containing three cells. The receptacle is convex on both sides, and kidney-shaped.

The seeds are numerous, and kidney-shaped also.

The SPECIES.

1. *Atropa caule herbaceo, foliis ovatis integris.* Linn.
Spec. Plant. p. 181.
Atropa. Linn. Hort. Cliff. 57. Roy. Lugd. 423.
Hort. Upf. 45. Dalib. Paris. 70.
Bella-donna majoribus foliis et floribus. Tourn.
Inst. 77. Boerh. Lugd. II. 69. Miller, plate 62.
Bella-donna dicta Solanum lethale. Hill. Herb.
Britan. p. 328. tab. 47.
Bella-donna. Clus. Pan. p. 503. Bod à Stap. p. 586.
Cat. Giffen. 142. Raii Syn. ed. 3. p. 265. Vail-
lant. Botan. Par. p. 20. Hall. Helv. 507. Dale
Pharmacol. 4^o ed. p. 72. Wilson. Synop. p. 122.
Solanoc ongener flore campanulato vulgatus, latioribus foliis. Hist. Oxon. III. p. 532. sect. 13.
tab. 3. fig. 4.
Solanum somniferum. Fuchf. 689. Icon. opt.
Solanum maniacum multis five Bella-donna. J. B.
III. p. 611.
Solanum melancerasos. C. B. pin. 166.
Solanum lethale. Ger. 169. emac. 340. Park. 346.
Raii Hist. Plant. 679.
Solanum majus five Herba Bella-donna. Matthioli.
Oper. Qmn. p. 756.
Solanum somniferum et lethale. Lobel. Adversar.
p. 102.
Deadly Nightshade, or Dwale.

2. *Atropa caule fruticoso*. Spec. Plant. 182.
Bella-donna frutescens rotundifolia Hispanica.
 Tourn. Inst. 77.
Solanum frutex rotundifolium Hispanicum. Bar-
 ril. Obf. 2. Icon. 1173.
Round-leaved shrubby Spanish Bella-donna.

3. *Atropa foliis sinuato-angulatis, calycibus clausis
 acutangulis*. Spec. Plant. 181.
Bella-donna flore magno violaceo. Hill. Herb.
 Brit. 329.
Alkekengi amplo flore violaceo. Few. Per. 724.
 tab. 16.
*Large violet-flower'd Bella-donna, or Deadly Night-
 shade*.

The first of the species here enumerated is the plant in question. The second has been found growing naturally in no other country than Spain. The third was first discovered by Father Feuillée in Peru, and is therefore only an inhabitant of the gardens in this part of the world.

The DESCRIPTION.

The root is perennial. It is pretty long, and divided into many branches of a brown colour, succulent, and of a disagreeable smell. The radical leaves are frequently a foot long, and five inches broad, of an oval acuminate figure, and not sinuated on the edges. The stalk rises to three or four feet: it is much divaricated and branched. The cauline leaves stand alternately upon it, in shape like the radical,

of a dusky-green colour on the upper part, and a paler green underneath, being a little hairy on both sides. The flowers stand on single footstalks, in the axæ of the leaves: they are large, of a campanulated figure, and striated, of a dusky-purple colour within, with a yellow variegated base; the outer surface of the flower is of a greenish red. After the flower succeeds a fine beautiful large berry, which is black when ripe. For the rest, take in the generical character.

Most of the old authors give us figures of this plant, which, tho' they convey a general idea of it, are yet scarce any of them exact. This fault in general runs thro' all, that I have had an opportunity of examining; namely, that the flowers and fruit are represented by much too large in proportion to the leaves. Morison's is perhaps one of the best among the old figures: it is, upon the whole, tolerable, but not accurate on account of the before-mentioned objection. Petiver's does by no means represent the plant justly, in that the axæ of the leaves are not properly filled up. The most accurate figure of all, that I have seen, is Mr. Miller's, in his plates adapted to the Gardeners Dictionary, which is undoubtedly taken from nature itself.

PLACE of GROWTH.

The Deadly Nightshade is found in many parts of Europe, especially in England and in Austria; and yet in our own country it is happily not very plentiful, inasmuch as our botanical writers usually reckon it among the *more rare* plants, and specify particularly the places where they have observed it.

Here

the latter, of two children, who perished by the same means (10).

Lobel (11) tells us, that the berries of this plant are present death; and informs us of some youth, who, after eating them, became stupified, and died as from an over-dose of opium.

Matthiolus (12) relates, from his own knowledge, of some children poisoned by the same means.

Among all the instances of the intoxicating nature of this plant, there is none more memorable than that mentioned by the Scotch historian Buchanan (13), of the destruction of the army of Sweno; which is quoted by almost all authors, who have wrote upon this plant. It is there said, that the Scots mixed a quantity of the juice of these berries with the drink, which, by their truce, they were to supply the Danes with; which so intoxicated them, that the Scots killed the greatest part of them while they were asleep. How far this anecdote is to be depended upon, or whether other concurrent circumstances ought not to be taken into the account, I cannot determine.

Our own herbalist Gerard (14) mentions the case of three boys in the Isle of Ely, who, having eaten of these berries, two of them died in less than eight hours; but the third, by drinking plentifully of honey and water, and vomiting after it, recovered.

(10) See Sennert. lib. vi. par. 7. cap. 9.

(11) Stirpium Adversar. p. 103.

(12) Oper. Omn. p. 754.

(13) Rerum Scoticar. lib. vii.

(14) Ger. em. p. 341.

Bodæus à Stapel, in his comment upon Theophrastus (15), tells us of two youths, that eat two or three of these berries, which they got in the Leyden garden, mistaking them for black currants: one of them perished, and the other recovered with great difficulty.

Simon Pauli relates two or three examples to the same effect (16). Wepfer gives us a circumstantial account of a child about ten years old, who was thrown into a great variety of convulsive symptoms after eating of this fruit: but proper care being taken by vomiting, and afterwards giving alexipharmics and anti-epileptic medicines, he recovered (17).

M. Boulduc (18) laid before the Royal Academy of Sciences at Paris, the case of some children, who, upon eating these berries, were seized with a violent fever, palpitations of the heart, convulsions, and lost their senses. One of them, a little boy of four years old, died the next morning.

Boerhaave has instances to the same effect (19): and it was the misfortune of Dr. Abraham Munting, a noted botanist and professor of physic in the university of Groningen, to have his own daughter poisoned with the berries of the *Bella-donna*.

It would be almost endless to recite all the instances to be met with upon this head. The German Ephemerides, the *Commercium Literarium*, and other periodical works, furnish us with farther proofs

(15) Page 586.

(16) *Quadripart. Botan.* p. 488.

(17) *Cicut. Aquat. Historia et Noxæ.* Basil. 1716. p. 228.

(18) *Histoire de l'Academie Royale.* 1703.

(19) *Hist. Plant. Lugd. Bat. Hort.* p. 510.

of the deadly quality of the Bella-donna; and they are unhappily corroborated by more recent instances in modern authors. The Gentleman's Magazine (20), Mr. Miller in his Gardeners Dictionary, and Dr. Hill in his British Herbal (21), exhibit to us several melancholy cases of this kind.

The effects of this plant have been so extraordinary, that several distinct treatises have been published professedly upon it. The most remarkable of these is that of J. M. Faber's, printed at Augsburg in 1677, under the following title; *Strychnomania explicans Strychni manici antiquorum, vel Solani furiosi recentiorum historiam*. In this tract the author has collected a number of cases from various hands, concerning the poisonous quality of the plant in question. In the year 1724. C. Sicelius published a treatise upon this plant, under the title of *Diatriba de Bella-donna*. Jenæ. 8vo.

MEDICAL HISTORY.

Who it was, that was bold enough to venture first upon the internal use of this plant as a medicine, I cannot say; chance very probably led to it, as in many other cases. In the mean time, there is reason to believe, that it is not altogether a modern practice. One would be led to think, by the accounts given us in Matthioli and Bodæus, that in their days its operation was very well known; and that they knew how to dose it very exactly, since they give us an

(20) For August and September 1747, and for Sept. 1748.

(21) Page 329.

account of tricks being played with it, by infusing the quantity of a scruple of the root in wine, and intoxicating people therewith. The former of these authors relates, that the distilled water from this plant, in a dose of about two or three spoonfuls, was exhibited by some people in inflammations of the viscera ; and, he observes, with good success. Parkinson seems to have transcribed this account, respecting this use of it ; but neither of them speak of it from their own knowledge. It may be questioned, however, whether this could act otherwise than as mere water ; since the principles with which this plant is endued, do not seem capable (if one may judge from its sensible qualities and effects upon those who have taken it) of rising in a still.

Mr. Ray (22), from the German Ephemerides, an. 13. obs. 64. presents us with the relation of a shepherd in Denmark, who administered an infusion of the berries in wine in the dysentery, which was there very common, and very obstinate ; adding, that it was attended with great success, not only restraining the flux, but carrying off the disorder by sweat. Mr. Ray observes further, that, correspondent with this practice, Conrad Gefner actually prepared a syrop from the berries, and gave it in dysenteric cases with great success. This account is found in Gefner's Epistles, and is quoted also by Dr. Haller, (23) when treating of this plant. Possibly its efficacy in these cases may be accounted for, from considering it merely in the quality of an opiate ; and

(22) Raii Hist. Plant. I. p. 681.

(23) Enumerat. Stirp. Helvet. p. 507.

therefore it cannot be adviseable to use it, when safer medicines are always at hand.

Its external use seems to be of as long a date as its internal; and it was on account of its cooling and repellent quality, that it came into credit as a fucus among the Italian ladies. Matthiolus recommends it in the erysipelas, the shingles, and other inflammatory disorders of the skin. The leaves, applied in the form of a cataplasm, are much celebrated by many writers, as of great use in resolving tumors, particularly of the breast, and even such as are of a schirrous and cancerous nature. Many of the old authors (24) mention this application of it, among other of the cooling and narcotic herbs; such as the common nightshade, henbane, hounds-tongue, &c. which it was usual to apply on such occasions. Mr. Ray informs us, that Mr. Percival Willughby experienced its efficacy repeatedly, in discharging hardnesses and cancerous tumors in the breast.

Its relaxing quality is very surprising, as appears by that memorable case related by the last-mentioned author, of a lady's applying a leaf of it to a little ulcer, suspected to be of the cancerous kind, a little below her eye, which rendered the pupil so paralytic, that it lost all its motion for some time afterwards: and that this event was really owing to that application, appears from the experiment's being repeated with the same effect three times.

The German physicians have gone much further: they have even ventured to give it inwardly in cancerous cases. Dr. Haller, when treating of the qua-

(24) See Forestus, Etmuller, and the old chirurgical writers.

lity of this plant, refers to Junker, and others of the modern physicians, as recommending the decoction of it with caution, that it be not given in such quantity as to cause sleep. So long since as the year 1739. there was a thesis published at Hall, by Michael Albert, in which the *Bella-donna* is proposed as a specific in cancerous cases. What other physicians patronize this use of it, I cannot say, having but little opportunity of consulting those academic pieces, which are of such eminent use in compilations of this kind. Thus much is certain, that its use, in such cases, rather gains ground; and the case, published in the French *Bibliothèque* (25), printed at the Hague, of an ulcerated cancer being radically cured by an infusion of the leaves of this plant in water, deserves particular attention, on account of its being so well attested. The case is extracted from an inaugural thesis of Professor Lambergen's, who was the physician concerned (26). The event was so singularly happy and successful in this instance, that we hope it will need no apology, if we give a particular detail of it.

The person afflicted with this miserable disease was a widow of 34 years of age, and mother of four children. She had but weak nerves, and had been subject to inflammatory disorders. She informed M. Lambergen, upon examining her, that she had had a quincy six times, which had twice ended in suppuration: that eight years before her right breast

(25) *Bibliothèque des Sciences et des beaux Arts pour les mois Jan. Fevr. Mars. 1755.*

(26) *Tiberi Lambergen Lectio inauguralis, sive Ephemeriden perfuncti Carcinomatis. Groning. 1754.*

had suppurated, and discharged much matter : that two years after it suppurated again ; and that at the end of another year both breasts underwent the same fate ; since when the right had remained schirrous, but was without pain, except when she handled it. She had suckled her youngest child about six months, when she was seized with a fever ; and the left breast (with which only she could suckle since the other had suppurated) soon swelled, inflamed greatly, was very painful, and soon became almost as large as a child's head. Dr. Lambergen being called in, ordered copious bleeding, and that the child should suck as little as possible. She took some medicines, and soon recovered.

A year passed after this without any bad accident ; when the lunar evacuations, which she had had from her 18th year, beginning to diminish, she felt a pricking pain in her left breast, and her right began to swell. Upon a fright, she had a fall, which accident increased both the pain and swelling ; and she had recourse again to Dr. Lambergen.

He found the tumors in her right breast much enlarged, and so connected together, as to feel like one large one only. On the upper part of the breast, upon the pectoral muscle, it felt rugged, unequal, and almost as hard as a stone. The patient complained of a constant itching in the part, and at times a pungent pain, which seemed to shoot from the armpit, and end in the tumor. Under this armpit the glands were hard and schirrous ; and the left breast was not exempt from the like indurations. A vein or two on the right breast was a little enlarged, otherwise no alteration. It was no hotter than common ;

mon; nor had it undergone any change of colour. To mitigate the pain of the schirrous, Dr. Lambergen ordered the following plaister :

℞ *Ung. Diapomphol.* ʒ ij. *Amalgam. merc. et Plumb.* ʒ iij. *Sperm. Cet.* ʒ j. *M.*

With this external application he prescribed likewise the following powders, to be taken night and morning, and gave directions relating to the non-naturals.

℞ *Coral. rub. Antimon. Diaphoret. illot. Sperm. Ceti* a ʒ ij. *Laud. gr.* vj. *M.* for 12 doses.

Under this method the pain remitted, but the tumor enlarged, and a little rising was observed on the upper part of it; and towards the nipple, where there was the least hardness, a small spot was perceived, which, at the next return of the catamenia, inflamed, and became the seat of the most excruciating pain. Dr. Lambergen, during this period, in the room of the powders, substituted emmenagogic pills, and ordered the pediluvium. She lost ten ounces of blood from the foot: and by these means the swelling of the breast diminished, and the patient suffered very little for some days. This truce, however, was but temporary: the rising on the upper part of the tumor began to inflame, itched intolerably, the pain returned, was almost perpetual, and insupportably pungent.

In this dreadful state was the patient, when Dr. Lambergen desired the late Dr. du Bois, Dr. Winter, physician to the house of Orange and professor at Leyden, together with Dr. Van Arum of Leewarden, physician in ordinary to the Princess dowager, to visit her. These gentlemen examined her many times, and unanimously agreed, that it was now no less than

a confirmed cancer. It was Professor Winter, who acquainted Dr. Lambergen, that he had heard M. Degner, a celebrated physician at Nimeguen, speak of the *Bella-donna*, as a sovereign remedy against inveterate schirri; adding, nevertheless, that he had never tried it himself.

In such a case as this, where death seemed inevitable, a dangerous remedy is to be preferred to none at all. Dr. Lambergen therefore determined to try it upon his patient; but, knowing the character, which the plant bore, he resolved to try the effects of it upon himself first. To this end, he poured ten tea-cups of water upon a scruple of the leaves, which had been gathered and dried three years: he let it stand all night lukewarm. Of this infusion he took half a tea-cup full, being the twentieth part of the whole, in the morning fasting; but perceived no effect from it. This determined him the next morning to double the dose; which produced a slight vertigo, and for an hour or two an uncommon dryness in his mouth. Being thus prepared, as he knew his patient had but a weak nervous system, he determined to begin with caution.

It was the 14th day of January 1745, that she took the first dose, being one tea-cup full. It had the same effect upon her, as it had had on her physician; and moreover rendered her pulse weaker and quicker than usual. For seven mornings successively she took the same dose, which, in general, produced the same effect. At the same time the plaster was renewed, with the addition of a few grains of opium. Under this method her pain was mitigated; but, before the latter end of the week, returned again
more

more frequently, and more acute; so that she was reduced to a most deplorable condition. The rising on the upper part of the breast became livid; the place near the nipple before-mentioned inflamed, and was very painful; and two little pointed risings were observed upon it, together with a slight fissure or opening. As the menstrual period was approaching, the infusion and the powders were omitted, and the pediluvium substituted. A mixture with crabs-eyes, *sp^t. nitri. d. and fyr. e mecon.* relieved the patient from some spasmodic complaints she had at this time, and the menses returned more copiously than ever. The 27th she took something more than a tea-cup of the infusion, being the first dose of the second scruple: her body was soluble; her breast less swelled, but the pain returned very acute, and seemed to terminate in the little callous eminence on the upper part of the breast, which now likewise became more pointed. The 28th she took the same quantity of the infusion. The two little pointed places near the nipple were now become two little holes, but had not discharged any matter. The other sore on the upper part of the breast was more livid still, and more painful, and had risen into two little whitish points. The powders were omitted this night, as they had been now and then at other times. The 29th, very little sleep the foregoing night, great pain from the upper sore, the holes near the nipple were become larger, and had run a yellowish matter. The same dose of the infusion as before. At night she had most acute pain from the upper sore. The 30th both sores were nearly in the same state. All remedies were

this day laid aside, except the infusion; of which she took a cup-full and an half; but her mouth soon became so dry, that she could scarcely swallow a little tea; and the vertigo was so violent, that she staggered: her sight was so weakened, that she could scarcely read. Notwithstanding this, she had no anxiety, nor nausea, nor pain; but her appetite was less, and her pulse quicker. The 31st all the last-mentioned symptoms continued the same: the two pointed eminences on the upper part of the breast were become two little holes likewise, and had discharged a few drops of good matter.

The 1st of February the upper sore had discharged but little matter, and that thinner than before. That near the nipple was become more livid, and the two holes were larger; but there had been no discharge for several days. The pulse and appetite were good, sleep natural, the body open. The pain, indeed, was continual, but less acute. The 2d, little alteration. The sores discharged but little. From this time they were dressed twice in a day with *Nutritum*, and over all the *Emp. Saturnin*. The pain was not so violent, but was felt in another place, which began to swell. From the 6th to the 28th better and worse: the pain more or less acute; and the catamenia passed without any bad effects. At the end of this month the schirrus all over the breast was much softened, and sensibly diminished. This was the opinion of Dr. du Bois and Dr. Winter, as well as of Dr. Lambergen.

The 1st of March an inflammation arose on the sole of the right foot, and extended up the leg about four fingers above the heel. It ended in two great blisters,

blisters, as if from a burn, which were embrocated with wine and oil, and nothing bad followed. To the 22d, the patient was better and worse. The menstrual period did not pass without some disturbance. The 26th she began with the infusion of the tenth scruple, and every thing went on for the better to the end of the month.

From the 1st of April to the 6th the ulcers were firm and dry; but the pain in the breast increased. Several blisters arose on the foot, along the leg, and even upon the thigh, on the left side. One upon the sole of the left foot, for 24 hours discharged an incredible quantity of thick whitish lymph. The pain from these blisters was beyond all she had felt before. It continued the 7th, 8th, and 9th; and new blisters arose on the thigh. The excoriated parts were all dressed with spirit of wine. During the disturbances from these new complaints, the breast was likewise painful, and swelled, tho' the ulcer near the nipple was dry, and the other discharged little or nothing. The 10th she had less pain both in her foot and in her breast. The upper sore was closed; the foot discharged less. From the 11th to the 15th, notwithstanding the weather was very cold, her pain still lessened. The ulcer remained firm, and the whole breast was softened: her foot mended; and all went on for the better till the 18th, when the ulcer on the upper part of the breast opened again in three places, and discharged a thick yellowish matter. The nipple of the left breast also became inflamed, and surrounded with pimples, which discharged a little lymph. In the mean time the cancerous breast was more painful than on the preceding days. The

M 2

19th

10th the pain less, tho' continual. Some discharge from the foot still; but the ulcers on the thigh were healed, and another blister arose. The 20th the upper sore on the breast closed again; but that near the nipple seemed to threaten another opening, and in fact it did, on the 24th, in three places. On the 20th the catamenia returned very copiously, and superseded the use of the pediluvium. The 25th she began with the infusion of the fourteenth scruple of the *Bella-donna*, which, it is to be observed, was scarcely ever omitted. The 26th a blister arose at the end of the fore-finger on the left hand, was very painful, and discharged a great quantity of serous matter. The next day both ulcers on the breast discharged a small quantity of lymph; otherwise the breast was less painful.

From the 28th of April to the 7th of May every thing went on for the better: the cancerous breast was almost without pain. The ulcers ran very little; but the excoriation and pain were much worse from the nipple of the left breast, which also discharged a great quantity of lymph. The 8th the upper ulcer on the right breast closed; but the other opened again. From the 8th to the 16th no change for the worse: on the contrary, the left breast was well; the right less painful, and discharged but very little. From the 18th to the 22d the menstrual period: all things on the mending hand; the ulcer healed, and the patient had little or no pain: but, from the 23d to the 27th, the pain returned something worse, and there was some discharge from the breast.

The *Nutritum* was now discontinued, as too emollient. The 27th the infusion from the eighteenth scruple of the *Bella-donna* was begun with.

From

From the 28th of May to the 12th of June the breast still painful : in the mean time, however, the ulcers remained firm and dry. The tumor and schirrosity of the breast diminished in such a manner, that, excepting its being a little bigger than the other, it had intirely resumed its natural form and colour. No indurations in the left breast, nor of the glands in the right armpit.

The 13th of June she took a journey, was absent some weeks, and returned in perfect health. Dr. Lambergen advised her, but in vain, to continue the infusion. Nevertheless, she was obliged now and then, when she felt pain, to have recourse to it, and was always relieved by it : and in the course of another year the remains of the schirrus were totally wasted.

It is now (1754) eight years since, and she has had no relapse, no pain, no hardness in her breast ; has married a second husband, by whom she has had a child, which she suckled. What more can be requisite to ascertain a cure ?

Thus we have given a detail of this memorable case ; wherein we see, that six drachms of one of the most poisonous vegetables that the world produces actually cured a woman, whom the most able physicians had given up as incurable ; and who must otherwise have finished her miserable days in the most deplorable sufferings.

It must not be omitted, that notwithstanding the daily use this woman made of the *Bella-donna*, she was not accustomed to it in the manner as people are who take opium. Dr. Lambergen always prepared the infusion himself, and never had occasion to make

it stronger than at the first, as the patient always
the like effects from the same dose.

So singular and happy an event, as attended Dr. Lar. bergen's administration of this plant, certainly merits the attention of the medical profession; and surely, one may add, entitles the medicine to future trials. And as the authenticity of the case will not be disputed, it is therefore greatly to be wished, that those gentlemen, who belong to the public hospitals, and others that have frequent opportunities of attending patients labouring under this deplorable disease, would give it a further trial. A cancer, even in its latent, but much more in an ulcerated state, is allowedly one of the most terrible and formidable disorders to which human nature is liable; and hath long been ranged, very justly, among the *opprobria medicorum*, instances of a radical cure being rarely met with: indeed, one of the first physicians (23) of our age tells us, that it is not known to have been cured at all, but by a total extirpation of the part; and all, who are conversant in physic and surgery, know very well, that that operation is frequently no security against its return.

I have here endeavoured, in as concise a manner as might be, to exhibit the history of this extraordinary plant. The being able barely to know and distinguish one plant from another, however praiseworthy in itself, ought not to be the only view of our botanical researches: we should do more, and

(23) Dr. Van Swieten Comment. in Aphor. Boerh.

endeavour

endeavour to investigate, in the most attentive manner, the properties of vegetable productions, in order to accomodate them to the various exigencies of human life.

Several classes of vegetables, from their merely herbaceous taste, and, as far as we can conjecture, from their other sensible qualities, seem to be formed by the great Author of nature principally for the nourishment of animals: but those plants, which are endued with principles so highly active, as, when taken in small quantities, to be able to put an end to animal life; such deserve to be more minutely inquired into, as under certain circumstances these principles, properly directed, may conduce to great and good ends. We should endeavour, therefore, diligently to inform ourselves, in what quantities, and under what circumstances, the poison ends, and where the medicine begins. In this respect we have certainly a notable instance in the history before us in Professor Lambergen; whose industry, more especially as it was attended with success, merits our greatest acknowledgements; inasmuch as he has informed us, with no small degree of accuracy and precision, that the plant under consideration, which is well known to be of a highly deleterious nature, and that even in a small quantity, may be so managed, as to be productive of good effects, not to be found possibly by any other means.

Some of the most efficacious medicines are such, as, being possessed of highly active principles, do greatly disturb the animal œconomy in their operation: nevertheless, however rough the *modus operandi* of any medicine be, if its efficacy by repeated
 trials

trials be approved and confirmed, this is so far from proving a discouragement to its use, that we ought to regard the discovery of such a one as a valuable acquisition to the province of physic, especially if it is applicable in desperate and obstinate cases. The *Bella-donna*, on the contrary, supposing future trials should prove it as happily successful as Professor Lambergen has experienced it, is a medicine of a different kind; inasmuch as its operation is mild, when compared with that, which attends the exhibition of many others: we should therefore have double reason to rejoice at the discovery.

XIII. *An Account of some of the Antiquities discovered at Herculaneum, &c. In a Letter to Thomas Birch, D.D. Secret. R. S. By John Nixon, A.M. F. R. S.*

Reverend Sir,

Read Feb. 24, 1757. **T**HE subject of this letter are some cursory observations made by me last spring, upon viewing the curiosities found at Herculaneum, and the places adjacent. I deferred putting them into any order, till I came to town, and had seen, by perusing the Transactions of the Royal Society, whether some abler hand had not already prevented me, and made any further communication needless: but as I now find, that no notice has been hitherto taken of several particulars, which, in my humble

humble opinion, deserved it, as tending to throw new light upon antiquity; I beg leave to trouble you with my thoughts upon them.

I shall begin with the museum in the King of the Two Sicilies' palace at Portici; wherein, amongst a great number of other ancient and valuable remains, are these that follow, *viz.*

I.

Several *tali lusorii*. The *tali* are supposed to have been known to the Greeks (1) by the name of Ἀτράγαλοι as early as the Trojan war. But as the monuments before us are undoubtedly Roman, I shall confine my remarks upon them to the usages received among that people; and being guided partly by what appears upon the face of these antiquities, and partly by what the Latin classics have delivered in general upon this subject, beg leave to observe, in the first place, that the *tali* had each of them but four sides, two broader, and the other two more narrow, on which they would ordinarily rest; as the rounding of their ends did not easily permit them to stand upon those parts. However, the possibility of such a position (tho' it did not occur to me to make the experiment with these pieces) may be deduced from a passage in Tully (2).

Further, with regard to the manner of distinguishing the several sides of the *tali*, some learned (3) writers

(1) Hom. Iiad. 23. v. 88.

(2) *Ut enim — si hoc fingamus, esse quasi finem — ita jacere talum, ut rectus assistat; qui ita talus erit jactus, ut cadat rectus* — Cic. de Fin. L. 3. §. 16. Ed. Verb.

(3) Vid. Dacier not. on Hor. L. ii. Od. 7. v. 25, &c.

ſpeak of it according to ideas taken from the faſhion of marking the modern dice, and (I may add) the ancient *teſſeræ* likewise: but, as I did not obſerve the traces of any engraving, painting, &c. upon the pieces under conſideration, it ſeems to me more probable, what others aſſert (4), that this diſtinction was effected by the different configuration of the ſides themſelves, and not by any numbers marked upon them. And concerning this notation, the common opinion is, that the appearances expreſſing *one* and *ſix*, as alſo thoſe repreſenting *three* and *four*, were oppoſed to each other reſpectively.

But leaving theſe (however probable) conjectures, we can with certainty determine the number of the *tali* uſed in this game to have been four; and like- wiſe, that among the various chances reſulting from them, the moſt fortunate one was that, wherein each of the ſides exhibited a different aſpect. The former of theſe circumſtances we learn from Tully (5), as we do the latter from Martial, who, in a diſtich ſent with a preſent of a ſet of *tali* to a friend, ſays,

*Cum ſteterit nullus vultu tibi talus eodem,
Munera me dices magna dediſſe tibi* (6).

It may further be collected from Horace, that the throw above deſcribed had the appellation of *Venus*: for when he intimates, that the preſident of the feaſt was elected by the *tali* (7), he muſt be ſuppoſed to

(4) Τὸ δὲ ἄνωμα τῶ κατὰ τὸ Ἀνδράγαλον Παιγματος Ἀντίθμης Δόξαν ἔχειν. Jul. Pollux. l. ix. c. 7.

(5) *Quatuor tali jacti caſu venerunt effiaunt.* Cic. de Div. l. i. §. 13. Ed. Verb.

(6) Mart. l. xiv. epig. 14.

(7) *Nec regna vini fortiſſe talis.* l. i. od. 4. v. 18.

mean the most favourable chance upon them. But he (8) elsewhere gives us to understand, that the chance, which determined that election, was called *Venus*.

Propertius is somewhat more explicite in assigning the title of this throw, as above; and at the same time informs us further, that the contrary (and consequently most unlucky) one was termed *canes*.

*Me quoque per talos Venerem quærente secundos,
Semper damnosâ subfiluere canes* (9).

Now it seems to be agreed among the antiquaries, (10) that *canis* on the *tali* was unity: and indeed this opinion is countenanced by Persius (11), who contrasts *canicula* with *senio*. If this be admitted, then the *canes* of Propertius must have been the chance, wherein all (or at least the greater number of) the *tali* came up (as we should express it) aces.

There have been several other conjectures proposed by learned writers upon this subject, which I choose to omit, for want of proper authorities from the classics to ascertain them. This is likewise the case with regard to the rules observed by the ancient Romans at this diversion. It is not at all improbable, that as we have several species of games upon the same set of dice, cards, &c. so they might have the same

(8) *Quem Venus arbitrum dicet bibendi?* L. ii. od. 7. v. 25.
Some think, that this cast was also named *basilicus* from the usage here mentioned. Sanad. in Loc.

(9) Prop. L. iv. el. 9. v. 18.

(10) Jul. Pollux. L. ix. c. 7. Lubin. on Pers. sat. 3. v. 49, &c.

(11) ——— *Quid dexter senio ferret,*

————— *Damnosa canicula quantum*

Raderet ———

Pers. sat. 3. v. 48:

variety on the *tali*: and if there were any laws established by custom for the regulation of this game in public, yet private parties might be at liberty to innovate at pleasure, and agree upon whatever terms of play were most agreeable to their inclinations or circumstances. In this light (according to (12) Erasmus) we are to consider the account, which Augustus gives of himself and his friends, in an epistle to Tiberius (13): *Inter cœnam lusimus γερωντικῶς beri et hodie: talis enim jactatis, ut quisque canem aut senionem miserat, in singulos talos singulos denarios conferebat, quos tollebat universos, qui Venerem jecerat.* And it is obvious to remark, that (upon this hypothesis) the critics, perhaps, need not have been so much embarrassed (as we find they have been) in endeavouring to reconcile this passage of Suetonius with that other of Persius (14) produced above.

I shall conclude with noting, that in order to prevent any fraud or slight of hand in managing the *tali*, it was usual to put them into a box (15), and, after shaking them together, to throw them out upon a table. Thus Martial introduces one of these *turriculæ*, as recommending its own usefulness for the purpose above-mentioned:

*Quærit compositos manus improba mittere talos,
Qui per me mittit, nil nisi vota facit* (16).

12) Dial. Ἀσπαλασις.

13) Sueton. C. Aug. §. 71.

14) Sat. 3. v. 48. See Prat. not. in us. Delph. in loc.

15) Hor. L. ii. sat. 7. v. 17.

(16) Mart. L. xiv. epig. 16.

However,

However, this caution does not seem to have been so universally observed, but that sometimes, *viz.* when the party consisted of ladies, it was (I presume, for a reason greatly to their honour) superseded. Thus, in one of the first paintings found at Herculaneum, and now in the royal apartments at Portici, we see a young female figure exhibited, as playing at this game, with one or more of the *tali* lying upon the back part of her hand, while the rest appear as having fallen off from thence towards the floor.

II.

A rule with four joints, each of which contained about 5 inches 9-tenths of our measure. I think there was another in two parts, which answered to the same proportion.

III.

A weight, inscribed on one side E M E, and on the other H A B E B I S.

IV.

A small *bolla d'oro*, which (after that in the late Dr. Middleton's collection, and another preserved at Rome) is the third known to be extant in Europe. As this ornament was worn by so great a number of young persons at Rome, and made of gold, which is so capable of resisting the injuries of the weather, moisture, &c. one cannot but wonder at the extreme scarcity of these monuments in the cabinets of the curious. The most probable way of accounting for this (according to (17) Dr. Middleton) is, that the

(17) Germ. Ant. Mon. p. 38.

value of the materials, of which these *bullæ* were made, induced the poor labourers, as soon as they had found one, to sell it to the first goldsmith they met with for its real value (however small it might be) by weight.

V.

A little figure like a Faunus, excepting that about the head it had something of the character of the minotaur, *viz.* large curls upon the forehead, and several muscular protuberances, or *tori*, under the throat.

VI.

A figure in relievo of a man sitting with a bowl in his hand, which has been thought a Socrates. And indeed the features of the face bear a striking resemblance to those of that sage expressed in ancient monuments; as the bowl might properly refer to the well-known circumstance of his death. But the other insignia are not so suitable to the character of the subject, as one could wish: for he holds, partly in his hand, and partly under his arm, a short staff full of knots, and curved at the end like a shepherd's crook, such as we find borne by satyrs in some Bacchanalian pieces: and the skin of a beast appears hanging from the seat of his chair.

VII.

An antique painting of a muse, with a *capsula* near her containing some volumes, from which hang labels shewing the titles of the works. The same representation appears in another painting kept in a different part of the palace. Signor Paderni
observed

observed to me, that these remains would help the curious to form a more certain idea of the manner, in which the ancients affixed titles to their volumes, than they have hitherto been able to obtain. The most complete description, that I can recollect, of an ancient book, with its appurtenances and decorations, is that of Martial addressed to one of his own.

*Faustini fugis in sinum? Sapisti.
Cedro nunc licet ambules perunctus,
Et frontis gemino decens honore
Piētis luxurieris umbilicis:
Et te purpura delicata velet,
Et cocco rubeat superbus index.* L. iii. ep. 2.

The *superbus index* in the last verse, curiously illuminated with scarlet, was undoubtedly the title of the book; but to what part of it it was annexed has hitherto been difficult to ascertain: for as (according to the paintings under consideration) it was inscribed on a detached piece of paper or parchment, it must soon have been lost from the book; especially if the latter had suffered by damps, or any other injuries similar to those, that have affected the volumes found in Herculaneum, of which not only the title, but even the umbilici, tho' consisting of more solid materials, as horn, ivory, &c. are intirely destroyed: so that no light could be had from the original antiquities with relation to this point. The only means, whereby the connoisseurs could form any conjectures in this case, must have been, I presume, from the fashion of books among the ancients, viz. their being long scrolls rolled round upon a stick with ornaments at each end, as described in the epigram produced.

produced above. This form required, that the books should be laid at their length upon the shelves, where they were deposited with either their side, or one of their ends, appearing outwardly. Now of these two positions the latter, which exposed the umbilicus to view, might be thought (all circumstances duly considered) the most convenient. To this part therefore it might with probability be conjectured, that the index or title was fastened; but the paintings mentioned above plainly demonstrate, that it actually was so.

Monf. Dacier says (18), that the titles of books were anciently inscribed upon the leathern covers, wherein they were wrapt, and which, by the means of thongs fastened to them, kept the volumes close and compact together. If that learned gentleman had supported this fact by proper evidences, then it must have been concluded, upon the joint authority of such evidences, and of the antiquities under consideration, that the practice of the ancients was, besides the title on the sides of the volume, to affix another on a label at one of its extremities. And indeed this additional notation (whatever we determine concerning its usefulness, while the books lay on a shelf in a library) must have been very necessary, when such books stood upright in a *capsula* (like those in the painting before us), where no part of them, but one end alone, could possibly be seen.

VIII.

Some pieces of fine paper, coloured red on one side, and black on the other, found upon the breast

(18) Not. on Hor. L. i. ep. 20. v. 2.

of a skeleton. Signor Paderni told me, that they had been viewed with great admiration by such of the virtuosi, as he had shewn them to ; and that their admiration proceeded from those fragments appearing not to be of the *charta papyracea*, but of that of silk, cotton, or linen. And indeed, if they should prove to have been made of any of the materials last mentioned, it would contradict the generally received opinion (according to (19) Montfaucon), that paper of silk or cotton, denoted by the common appellation of *charta bombycina*, was first found out in the 9th century ; as that composed of linen rags (*ex linteolis contritis et aquâ maceratis*, as Pancirollus (20) expresses it) was about the 12th ; and that the former supplied the place of the *charta papyracea* in the east, as the latter superseded the use of it in the western parts of the world.

A flat piece of white glass, taken off from towards the extremity of the sheet, as appears from the curvature and protuberant thickness of one of its sides above the other parts. I have several observations by me, with regard to this fragment, which I have not yet had leisure to digest. I shall therefore proceed to the other parts of this collection.

To enter into a detail of the paintings found at Herculaneum, and deposited in a different part of the palace at Portici, would be tedious, as their

(19) Mem. lit. de l'Acad. des Inscip. V. 9.

(20) Rerum Mem. L. ii. tit. 13.

number, when I saw them, exceeded 800; and it would be superfluous, as the principal of them will soon make their appearance in the world by prints taken from them, and executed in a manner, which (as far as I could judge by the specimens shewn me) will in no-wise discredit the originals, I shall therefore only mention two of them, *viz.*

I.

Theseus with the Minotaur dead, and lying on his back at his feet, while several Athenian youths are embracing the knees, and kissing the hand, of their deliverer. We may observe, that the fabulous being above-mentioned appears in this piece with the intire body of a man, and only the head of a bull, which agrees with the manner, in which he is represented in an antique sardonix of Greek sculpture in the cabinet at Vienna, and in most of the works of the ancient artists. Tho' I have by me the copy of an antique gem, wherein the Minotaur is exhibited as standing in the center of the famous labyrinth, and having below the body of a bull as far as to the waist, and from thence upwards an human form: which representation is further countenanced by Ovid, who describes that monster, as

Semibovemque virum, semivirumque bovem.

Art. Am. L. ii. v. 12.

II.

Chiron and Achilles. The latter of these is standing, and has a *plectrum* in his right hand: the former seems to embrace his noble pupil with his left arm, and with his right hand to strike the lyre, as teach-
ing

ing him to play upon that instrument. But the most remarkable circumstance in the figure of Chiron is his reposing his hinder parts on his left haunch upon the ground. Yet this attitude, as well as the other particulars mentioned above, is expressed in an antique gem, of which I have seen a copy at Rome.

I shall conclude this paper with an account of the statues, which stand in several rooms adjoining to the unfinished part of the palace, and were found (as to the far greater number) at or near Herculaneum.

In the First Room.

An equestrian marble statue of M. Nonius Balbus the elder, which is intended to be placed in a large entrance on the east side of the palace, to answer to that of his son, which is already set up on the other side, facing the bay of Naples.

In the Second.

Nero and Germanicus, considerably larger than the life, but squeezed somewhat flat by the weight of the lava, or other ruins, with which they were once overwhelmed.

A man in a sacrificing habit.

Two others in the toga, and two women in the palla.

All these are of bronze.

Statues of marble deposited here are the following, viz.

At the entrance, a matron larger than nature, with strong expression in her face.

Two colossal trunks in a sitting posture.

Three statues of one of the Agrippina's.

A Roman matron, or empress, with remains of red painting on the extremities of her palla.

Three other matrons.

In the Third Room.

Bacchus. A muse. A fragment of a statue in the pallium. A fine statua togata with the head veiled, larger than the life.

Another very remarkable figure, whose face resembles in beauty that commonly attributed to Venus, tho' the dress and other insignia plainly indicate a Pallas: for her head is covered with an helmet, below which her hair falls down long and dishevelled. Her left arm is enveloped with her ægis, which is large and expanded, so as to form a kind of mantle. Her garments are thin, and fit close to her body in strait plaits. She is in a posture of running, or striding, with her feet at a considerable distance from each other, and her arms extended different ways; an attitude strongly marking the utmost eagerness and haste.

Next appears a Vertumnus. A fine figure of a philosopher. Volumnia and Veturius. A lady with a thin stola. A Venus. A boy of exquisite workmanship. A small statua togata.

In another part is a Faun of bronze, reclined, with his right hand lifted up, and his leg extended. This figure (as we were informed) was found accompanied with seven others of the same metal, which now stand in another chamber, viz. two young men in a running a posture; four females somewhat resembling

resembling vestals in their habit, excepting that all their heads were uncovered, and those of two of them were adorned with *vittæ*, or filets. Lastly, a young man of a small size, cloathed, with his arms somewhat extended.

There remains but one more figure to be taken notice of in this collection, *viz.* that of Serapis, with Cerberus at his right hand. Ancient writers (21) enable us to account for this appearance, by informing us, that Serapis (besides his other characters of *Æsculapius*, *Sol*, *Osiris*, and *Jupiter*) was accounted the same as *Dis Pater*, or *Pluto*. Upon this hypothesis none can doubt of the propriety of *Cerberus's* attending upon this deity in the figure before us, as well as in three others given us by *Montfaucon* (22).

If we desire to enter into the mystical reason of this representation, we may learn it from *Porphyry*, *viz.* that *Serapis* (23), being the same as *Pluto*, had dominion over the evil *dæmons*; and that those beings were figured by a dog with three heads; meaning the *dæmon* subsisting in the three elements of water, earth, and air.

Give me leave to add further, that I find, by my journal, that upon viewing this figure, I took notice of a dissimilitude in the heads of it: but as it did

(21) *Deum ipsum (Serapidem) multi Æsculapium — quidam Osirim — plerique Jovem — plurimi Ditem patrem insignibus, quæ in ipso manifesta, aut per ambages, conjectant.* Tac. Hist. L. iv. Εἰς 2 ὅς, ἐν Αἰδῆς, εἰς Ἥλιος ἐστὶ Σάραπις, Oraculum Apollinis apud M. A. Cauf. Museum Rom. vol. ii. §. 6. tab. 13.

(22) Antiq. T. ii. P. 2 pl. 121, 122.

(23) *Porphyry*, apud *Euseb. Præp. Evang.* L. iv, c. 23. Τὸς δὲ πρῶτος διὰ τὸν ὄντα ἐκ ἐκκλῆ ὑπὸ Σάραπιν ὑποπλάττουσιν, &c.

not then occur to me, that they were ever expressed in any other form than the canine, I did not examine minutely into the difference : but, upon recollection, I am now inclined to think, that that monster might have the heads of three several animals in this piece, as he has in another, given us by (24) Montfaucon : which mode of exhibiting him was (according to that learned (25) antiquary) invented by the Egyptians ; a circumstance not to be wondered at in a people, whose imagination teemed so plentifully with monstrous ideas of all kinds, as theirs is known to have done.

To the same original we may refer the serpent twisting round Cerberus in this monument ; as we see two of the same species encircling his heads and body in that mentioned above (26). As I know no particular relation, that the serpent bears to Serapis, considered as Pluto, I can regard it here only as a sacred symbol in the theology of the ancient Egyptians ; and, as such, properly attributed to an attendant of one of their chief divinities.

I shall trouble you but with one more observation upon this article, *viz.* that (if I may trust my memory for a particular omitted in my notes) this is the statue, which being the principal one found in an ancient magnificent building discovered about seven years ago, at (27) Pozzuoli (in conjunction with other circumstances) occasioned it to be called The Temple of

(24) Suppl. T. ii. L. vi. c. 10. Tab. xlviii.

(25) Montfaucon, *ibid.*

(26) Montfaucon, *ibid.*

(27) Vid. Observations sur les Antiquités d'Herculanum, &c. par Mess. Gochin & Bellicard, p. 83. Paris 1755.

Serapis. As this place seemed greatly to merit the attention of the curious in antiquity, we procured a plan of it, drawn by a native, who has free access to it; and (if I thought it would be acceptable to that learned Society, of which I have the honour to be a member) the said plan should wait upon them, accompanied with some observations upon it by,

S I R,

Your most obedient,

London, Feb. 24.
1757.

humble Servant,

John Nixon.

P. S. A long room is designed to be fitted up in the King's palace at Portici, for the reception of all the antiquities found at Herculaneum, &c. This apartment will be lighted by thirteen windows on the side towards the Cortile, and adorned with forty columns, partly of verde antique, partly of alabaster with brownish veins, and other beautiful marbles, found in divers parts of the King's dominions. Between every two of these columns will be placed a group, statue, or bust. The compartments in the walls will contain the ancient paintings. The other curiosities are to be deposited in cases made for that purpose; and the pavement will consist intirely of the finest pieces of Mosaic work, that have been found in Herculaneum, or any places within the Neapolitan state.

XIV. *An Account of the Effects of a Storm of Thunder and Lightning, in the Parishes of Looe and Lanreath, in the County of Cornwall, on the 27th Day of June, 1756. Communicated to the Rev. Jeremiah Milles, D.D. F.R.S. in two Letters, one from the Rev. Mr. Dyer, Minister of Looe, and the other from the Rev. Mr. Milles, Vicar of Duloe, in Cornwall.*

Read Feb. 24,
1757.

ON Sunday the 27th of June last it grew on a sudden as dark as a winter evening: soon after, the lightning began to flash, and the thunder to roar. The claps were near, and extremely loud; and the lightning was more like darting flames of fire, than flashes of enkindled vapour. Happily no damage was done to the town of Looe, which lies very low; but at Bucklawren, a village situated on the top of a hill, about two miles from hence, a farm-house was shattered in a most surprising manner. The house fronts the south. The windows of the hall and parlour, and of the chambers over them, which are in the front of the house, are shattered. The dairy window is the only one on the west side of the house. The chimnies are on the north side; and at the south-west corner there is a row of old elms on a line with the front, the nearest of which is ten feet distant from the house. The lightning seems to have had a direction from

from the south-west to the north-east. It first struck the bevelled roof of the south-west corner, near the eaves of the house; made a large breach, and tore up the floor of the garret, near the place where it entered, and descended by the west wall, in oblique lines, into the chamber over the parlour; but not having sufficient vent that way, it darted in a line from S. W. to N. E. against the north wall of the garret, where meeting with resistance, it broke down the floor near the north wall many feet wide, and carrying the ceiling of the parlour-chamber before it, ran down by the wall of that room in direct lines. Where it descended on the west and north walls it made large and deep furrows in the plaister, and even tore out the stones and mortar. A large splinter was struck off from the bed-post contiguous to the north wall, and the bed was set on fire. The chimney-piece was broken into many parts; the window-frame was moved out of the wall, every pane of glass was broken, the under sash was torn in pieces, and a large piece of the chimney-board was thrown out of the window against an opposite garden wall, about 20 feet from the house. As the lightning shot thro' the window, it found a small cavity between the wall and the slating with which the wall is covered, where it burst off the slates as far as it continued in a direct line downward, and threw them at a great distance from the house. Notwithstanding this dreadful havock, the force of the lightning was not spent; the window gave it not a sufficient discharge. From the chamber over the parlour, it descended by the north wall to the room under it, which is wainscotted, tore off the cornice the whole breadth of

the room, and some mouldings from the wainscot ; broke the glasses and Delft ware in the beauffet ; shivered the shelves of a bottle-room ; and, ripping off a small stock-lock from the door, burst it open, and made its way chiefly thro' the window, the frame of which was moved from the wall, and the glass shattered to pieces. Near the bottle-room there was a hole struck in the partition-wainscoting, which divides the parlour from the hall, about eight inches long and an inch broad : through this crevice the lightning entered the hall, which serves at present for a kitchen, and meeting with some pewter in its way, it flung it from the shelf about the room ; threw down a large iron bar, that stood in a corner, and which seemed to have a trembling and desultory motion ; carried the tongs into the chimney, and threw a tea-kettle, that stood there, into the middle of the floor ; moved a large brass pot out of its place, which was under a table ; and then darted thro' the windows, carrying away a pane of glass intire out of the upper sash to the distance of many feet. The mistress of the house and her son were sitting at this window. They were the only persons in the house, and providentially received no hurt. Some part of the lightning found a way between the door and door-case of the hall. The door is pannelled : and the lightning, in passing thro', penetrated into a close mortise, and split off a large splinter from the outside of the door, close to the tenon. In its course it left a smoky tinge on the wall and timber, like that of fired gunpowder. A sulphureous smell remained in the house many hours. Another (or probably a part of the same) flash of lightning struck

struck the dairy window, melted the lead, and burnt the glass where it penetrated, and set the window-frame on fire. From thence it darted in a line from S. W. to N. E. downward, made a large hole in a plaistered partition near the floor into the barn, shattered a large paving rag-stone in pieces, and tearing up the ground, I suppose, sunk into the earth. The elms were affected with the lightning, particularly that nearest the house, from the top of which to the root appeared large furrows in the moss, which grew on the bark, in some places in an irregular spiral, but for the most part in a perpendicular line; and from the root of it the ground was torn up in furrows, as if done with a plough-share, about six feet long, the furrows gradually lessening according to their distance from the tree. All this was done instantaneously. How amazingly swift, subtle, and powerful is the force of lightning! I am,

Reverend Sir,

Your most obedient Servant,

James Dyer.

A Letter from the Rev. Mr. Milles on the same Subject.

ABOUT four of the clock on Sunday afternoon the same day that the lightning struck the farm house at Bucklawren, it fell upon another house called Pelyne, in the parish of Lanreath, about six miles distant. The house fronts the east. The chimney, which is at the north end, is cracked, and

opened about two or three inches wide, from the top to the roof, where it entered the flating thro' a small hole on the eastern side; forced its way thro' the upper chamber, where it melted an old copper skillet, a pair of sheepshears, and some odd brass buckles and candlesticks that lay on the wall; consumed the laths adjoining, and then made its way thro' a small crevice in the upper part of the window. Another and more severe part of the same lightning descended the chimney; struck two women down, who were sitting on each side of it, without any further hurt; overturned a long table, that was placed before the window in the ground room, upon two men, who were sitting on the inside, with their backs towards the window. One of these men was miserably burnt in his right arm. The lightning seems to have struck him a little above the elbow, making a small orifice about the bigness of a pea: the burn from thence to the shoulder is near an inch deep. His right thigh was likewise burnt on the inside, and the outside of his right leg, from a little below the knee, quite over the ankle to his toes. Both knees were burnt across slightly, and his left thigh. His shirt-sleeve, and the upper part of his waistcoat, were reduced to tinder: the buckles in his shoes were melted in different parts, and in different directions. He has not been able to use his arm since; and is under the care of a surgeon, who has reduced the wound to a hand's breadth, which was in the beginning advancing fast towards a mortification. The other man was but slightly wounded. The lightning afterwards found its way thro' the window in three different places; melted the glass, leaving a smutty tinge, like that of fired

fired gunpowder. A boy, about ten years old, son to the under-tenant, was also struck down, as he was standing at the door, but not hurt. The father and his daughter felt no ill effects; but saw the lightning roll on the floor, and thought the room was on fire.

XV. An Account of the Peat-pit near Newbury in Berkshire; in an Extract of a Letter from John Collet, M. D. to the Right Reverend Richard Lord Bishop of Orlery, F. R. S.

My Lord, Newbury, Decemb. 2, 1756.

Read Feb. 24,
1757.

NOW I am mentioning the peat, I beg leave to assure your Lordship, that tho' some persons have asserted, that after the peat has been cut out, it grows again after some years; yet this is not true of the peat found here, none of the peat-pits, which were formerly dug out, and have lately been opened again, affording the least reason to justify such an opinion; but, on the contrary, the marks of the long spade (with which they cut out the peat) are still plainly visible all along the sides of the pits, quite down to the bottom; and are now as fresh as if made but yesterday, tho' cut above fifty years ago: which shews also, that our peat is of too firm a texture to be pressed together, and to give way, so as to fill again the empty pits: which perhaps may be the case in some of the mosses, where the pits are found after some years to be filled up again.

The

The town of Newbury lies north and south, in the shape of a Y, cross a valley; which valley runs east and west, and is here about a mile broad, the river Kennet running along the middle of it. The peat is found in the middle of this valley, on each side of the river, extending in all from between a quarter of a mile to about half a mile in breadth; and in length, along the valley, about nine miles westward, and about seven eastward; and I believe much further, tho' not yet discovered, and perhaps with some intermissions.

The ground it is found in is meadow land, and consists chiefly of a whitish kind of earth: under this lies what they call *clob*, being a peat-earth, compounded of clay, of a small quantity of earth, and some true peat: it is from four to eighteen inches thick; and where the earth above it is but thin, it is sometimes full of the roots of plants, that grow on the surface of the ground: and if the meadow also be moorish, the sedge and flags will shoot their roots quite thro' it into the true peat, which lies directly under this clob.

The top of the true peat is found at various depths, from one foot to eight feet below the surface of the ground; and the depth or thickness of this peat is also very different, from one foot to eight or nine feet, the ground below it being very uneven, and generally a gravel. My friend Mr. Osgood has dug two feet into this gravel, to see if any peat lay below it, but could not find any.

The truest and best peat has very little (if any) earth in it; but is a composition of wood, branches, twigs, leaves, and roots of trees, with grass, straw, plants,

plants, and weeds ; and lying continually in water makes it soft and easy to be cut thro' with a sharp peat-spade. The colour is of a blackish brown ; and if it be chewed between the teeth it is soft, and has no gritty matter in it, which the clob has. It is indeed of a different consistence in different places, some being softer, and some firmer and harder ; which may perhaps arise from the different sorts of trees it is composed of.

To get at the peat, they first dig up the surface of the ground till they come to the clob, throwing the earth into the empty pits, from which they have already cut out the peat : they then dig up the clob, and either sell it to the poor for firing, or lay it in heaps, to burn to ashes, to be sold to the farmers. Then they cut out the true peat, with a peculiar kind of spade, in long pieces, vulgarly called long squares, about three inches and a half broad every way, and four feet long, if the thickness of the peat will allow that length : and as they cut it out in long pieces, they lay them in a regular order carefully, in rows upon the ground, to be dried by the sun and wind. If the peat be thick, when they have cut one length of the spade for some distance, they return again, and cut down another length of it (or four feet), and so on, till they reach the gravelly bottom, if they can sufficiently drain it of the water, which continually comes in, tho' proper persons are employed to pump out as much of the water as they can all the time. As the peat dries, and is turned by persons appointed for that purpose, to dry it the better, it breaks into smaller lengths, and then it serves not only the poor ; but many other persons, for firing, and gives a good
heat.

heat. It is sold for about ten shillings a waggon-load, delivered at their houses in the town. The ashes also prove very good manure for both grass and arable land; and the farmers give from four pence to six pence a bushel for them, which renders this firing very cheap.

Great numbers of trees are plainly visible in the true peat, lying irregularly one upon another; and sometimes even cart-loads of them have been taken out, and dried for firing: but the nearer these trees lie to the surface of the ground, the less sound is the wood: and sometimes the small twigs, which lie at the bottom, are so firm, as not to be easily cut thro' with the usual peat-spade. These trees are generally oaks, alders, willows, and firs, besides some others not easily to be known. The small roots are generally perished; but yet have sufficient signs to shew, that the trees were torn up by the roots, and were not cut down, there being no sign of the ax or saw; which, had they been felled, would have been plainly visible.

No acorns are found in the peat, tho' many cones of the fir-tree are, and also a great number of nutshells. They are all of a darkish colour; and the nuts are hollow within, and some of them have a hole at the broad end.

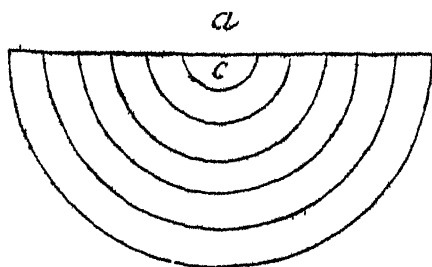
A great many horns, heads, and bones of several kinds of deer, the horns of the antelope, the heads and tusks of boars, the heads of beavers, &c. are also found in it: and I have been told, that some human bones have been found; but I never saw any of these myself, tho' I have of all the others.

But

But I am assured, that all these things are generally found at the bottom of the peat, or very near it. And indeed, it is always very proper to be well and faithfully informed of the exact depth and place, where any thing of these kinds is found; whether it is in the earth above the peat, or in the clob; or in the true peat, or at the bottom of it; which will greatly assist us in forming a just judgment of the real antiquity of the things that are found, or at least of the time they have lain there. Besides this, as they formerly used to cut out the peat in large plots here and there, leaving spaces full of peat between those pits (whereas now they draw off the greatest part of the water by pumps, and so clear out all the peat regularly as they go on); so it must be carefully observed, whether whatsoever is found here be dug out of these old peat-pits, or not; for axes, and other things, may have been formerly dropt into these pits, before they were filled up again with earth, and may now be dug out of them again. My father has now in his possession an iron hatchet, not greatly differing from the modern form, which was found lying flat at the very bottom of the peat: it was covered with a rust near half an inch thick, and the handle was to it, which seemed to be of beech-wood, but was so soft, that it broke in bringing it up: but as the person is dead, who found it, I can't say whether it lay in an old peat-pit, or no.

Mr. Osgood found, some years ago, an urn, of a light brown colour, and large enough to hold above a gallon, in the true peat, about eight or ten feet from the river, near a mile and a half west of this town, in Speert-moor. It lay about four feet below

the level of the ground, and about one foot within the peat; and over it was raised an artificial hill, about eight feet higher than the neighbouring ground; and as the whole hill consisted of both peat and meadow-ground intermixed together, it plainly appeared, that the peat was older than the urn; and that the persons, who raised the hill, must first have dug a large hole in the peat, to bury the urn there, and so formed the hill of the peat and meadow-ground mixed together. Round the hill, where the urn lay, they had made also many half-circular ridges, with trenches between them, one beyond another, in this manner:



Where *a* is the river, and *c* the hill; and the half circles shew some of the ridges, the number of which Mr. Osgood has now forgot. The urn was broke by the peat-spade, and it came up only in small pieces, so that nothing was found in it; and no body happened to be there at that time but the peat-cutters.

No coins of any sort have been found in the peat. But there may, perhaps, be a variety of things at the bottom of it: but as the peat is always full of water, which is never quite drained off, so it is not an easy matter to examine the bottom.

I beg leave to assure you, my Lord, that I am,
with great respect and esteem,

Your Lordship's most obliged,
and obedient humble Servant,

John Collet.

XVI. *An Account of the Alterations making
in the Pantheon at Rome: In an Extract
of a Letter from Rome to Thomas Hollis,
Esq; Communicated by John Ward, LL.D.
R. S. Vice-Præs.*

Read Mar. 3.
1757.

A Project was lately laid before the government by Paolo Pofi, an architect, for modernizing the inside of the Pantheon, and unfortunately approved. In consequence of which, the dome has been already cleaned, and rough cast; and the remainder of the lead taken away, which served as a lining to the silver work, that originally covered it. The vestiges of the cornices, and other ornaments of the silver work, were still discernible in the lead, which was fastened by very large iron nails. All this was effected by a moveable scaffold, that was fixed to the bronze cornice of the open circle above, whereby the temple is illuminated, and descended to the cornice of the Attic order, being as curious in the contrivance, as detestable for the purposes intended by it. It is true, we could not before see the dome in its pristine glory;

but we had the satisfaction of viewing the traces and remains of what it had been. Nor could the adepts in architecture sufficiently admire the skill and sagacity of the builder, who, composing it of a number of small arches, which together formed a kind of net-work, and filling up the intervals between with pumice-stones and mortar, gave it that strength and lightness, whereby it has probably stood so many ages.

The evil would be comparatively small, had the project extended no farther, than what has been related ; but they are now busy in removing the Attic order, to make room for a new invention, suitable to the trifling taste, which at this day prevails. And not content with that, they think of taking away the ancient pavement ; and, what is still worse, its peculiar beauty, the open circle at the top, to place a lantern instead of it, as is usual in modern cupola's.

You had the good fortune, Sir, to view this remarkable temple, in that state, wherein it was left by the ancient barbarians : but those, who see it hereafter, will find it in a much more deplorable condition, stripped of its precious marbles and ornaments ; and so disguised by modern alterations, that the noble form given it by Agrippa will be no longer distinguishable.

It is said Il Signor J. B. Piranesi, the architect, who published the antiquities of Rome, and divers ingenious works of that kind, has taken accurate plans of the Attic order, and every other particular relating to it. These he proposes to engrave and publish, with exact explanations annexed to them ;
together

together with a plan of the whole, as he believes it appeared in its original splendor and perfection ; that posterity may not be deprived of informations, which are of so great benefit and importance to all lovers of architecture. It is also said, that the engineer [carpenter], who invented the scaffolding, has made an exact model of it for him ; which he intends to publish as a part of the work before-mentioned.

XVII. *An Account of a new medicinal Well, lately discovered near Moffat, in Annandale, in the County of Dumfries. By Mr. John Walker, of Borgue-House, near Kirkudbright, in Scotland.*

Read Feb. 10, & Mar. 3, 1757. **T**HIS mineral spring was found out by one Mr. Williamson, a few years ago, when he was overseeing a mine, which was at that time carrying on in its neighbourhood. It is situate about four miles distant from Moffat, in the bottom of a deep scar, which is on the west side of a large mountain called Hartfell, from which it has acquired the name of Hartfell-spaw. This scar is a part of the mountain, thro' which a small stream of water has worn its way to a considerable depth ; by which it has laid open, and exposed to view, the strata of the earth on each side : and in the bottom of this scar, and near to the brink of this small brook, the mineral water springs up. When

When I saw it, it consisted of two springs, which were very well ordered by Mr. Williamson, so as to run from two wooden spouts, immediately at their rise out of the earth; which indeed must be of very great advantage to all mineral waters: and I am persuaded there are many, whose medical qualities are greatly impaired by falling into reservoirs, and continuing in them for some time after they spring from the earth. The one of these springs was situate about ten or twelve yards further up the brook than the other; and they were then distinguished by the names of the upper and lower spring: but I have been since informed, that their situation is now altered. Each of these springs did at that time run nearly the same quantity of water, which, as I thought, was above an English quart in a minute, and that during a season of very dry weather.

As there are many instances of mineral waters springing out of the earth very near each other, which at the same time are impregnated with very different principles; it therefore seemed not at all improbable, that as these waters did appear to run, for some part of their course, in different channels, they might in some respects be different from each other. And this suspicion I found not to be altogether groundless with regard to these springs, as will be shewn afterwards. For which reason it may be observed, that the following trials were all made upon the water of the upper fountain, except where the other is particularly mentioned; and also that they were made within 24 hours after the water was taken from the spring, being carried to Moffat in bottles carefully sealed.

According

According to what may be inferred from the following experiments, it may be premised, that this water appears to contain in it a large proportion of iron, but in two different forms; and an aluminous salt, which is conjoined with a terrestrial principle.

As the contents of several mineral waters have been the cause of many different opinions, and of great disputes among physicians and chymists; as the inquiry I made into the principles of these waters, which I am now considering, was not performed with that nicety and exactness I could have wished; and as I am persuaded, that to dogmatize in any branch of philosophy can never tend to its advancement; I shall not therefore pretend to determine with certainty in any part of this subject, where the contrary opinion can be admitted with the least degree of probability. These trials are indeed but few and imperfect, and are no way sufficient to form an exact account of this mineral water; yet I believe they may afford some conclusions, which may be serviceable in compiling a more compleat history of it. They render it pretty evident, that the above-mentioned principles are contained in these waters: and tho' I will not pretend absolutely to exclude all others, yet I must say, that, by what inquiry I made, I could not observe them to be in the least impregnated with any other kind of mineral substance.

After a good deal of observation upon the water of this Spaw; and after many fruitless attempts, which I have at different times made upon several other waters of the chalybeat kind in Scotland, in quest of the volatile spirit, which has been commonly attributed to them; I must own, that I have been
induced.

induced to think, that there is no such thing exists in these waters at all. What has been generally called the spirit of steel waters, seems to me to be very unintelligible; altho' the existence of it in these waters has been asserted by all the writers on this subject, which I have yet had occasion to see. The spirit of a mixed body is commonly taken to be a subtle, penetrating, light, and volatile substance, more susceptible of motion than any other of its parts, and most easily separable from them by avolation. But that any chalybeate water contains such a substance, I think has never been made evident, unless where the water has been found to be impregnated with some other mineral principles. Some steel waters, I believe, contain a large proportion of air, whose elasticity may occasion it to break forth with an explosive force; some others there are, which contain a volatile and sulphureous halitus; and to one or other of these two causes, or to some other mineral principle, I think most of the phænomena may be referred, which have been attributed to a ferrugineous or vitriolic volatile spirit.

As the first thing observable in a mineral water is its outward form, we must therefore take notice, that the water of this Spaw equals the clearest spring-water in transparency; and is likewise as free of any colour or odour: yet its taste is very strong, and may be discerned to be compounded of a sweet, subacid, and astringent taste. Its sweetness and acidity appear sensibly to arise from alum; and its high styptic and astringent taste does as evidently proceed from that mineral salt, joined with some earthy or ferrugineous parts. I must likewise observe, that when

I first

I first compared the taste of these two springs, I could plainly discern, that the water of the lower spring was more acid, and less astringent, than that of the upper one; and, on the contrary, the water of the upper spring seemed more astringent, and less acid. This induced me to think, that the mineral parts, which caused the acid and astringent tastes, were mixed in the waters of these two springs in different proportions. And what I observed of them afterwards still confirmed this conjecture.

But, in order to give some evidence for the existence of the above-mentioned minerals in the waters of these springs, we shall consider them separately, by relating the experiments, which seemed to indicate, that they are contained in these waters in a very considerable proportion, and by offering some conclusions, which may be reasonably drawn from them.

And as the first trials were made in quest of iron, it may perhaps be most proper to consider it in the first place.

Experiment 1. Some pieces of galls being added to equal quantities of the water of the two springs, an exceeding deep and bright blue colour was immediately produced in the water of the upper spring, which in a little time turned to a perfect black. The water of the lower spring, tho' indeed it was turned of the same colour, yet was not of so deep a shade, but was somewhat lighter than the former. The tincture of galls caused the same appearances.

2. A tincture of balauftine-flowers produced the above blue colours in both waters.

3. A quantity of the water being thoroughly tinged with galls, was allowed to stand 24 hours: being then filtrated thro' brown paper, the water, tho' almost quite colourless, would not again receive any tincture with galls.

4. After elixation the water became of a turbid yellow colour with ochre, and afforded very little tincture with galls.

5. A solution of sal Martis, chemically prepared, being mixed with galls, immediately turned of a bright dark blue colour, exactly similiar to that produced in the water.

6. A solution of common and rock alum was no-ways changed in its colour with galls.

7. A solution of sal Martis and alum being mixed in equal quantities, the mixture was turned of a bright blue colour with galls; yet not of so deep a hue, but of a more diluted colour than the solution of sal Martis, without alum.

From these experiments we must first of all observe, that the colour, which these waters afford with galls and pomegranate-flowers, is very uncommon. The more iron, that any mineral water contains, it will afford the deeper colour with such astringents: but tho' I have tried this experiment upon a great many of the ferrugineous waters in Scotland, and also upon the water of some of the foreign Spaws, I never observed one, that afforded so deep a colour as this, which we now consider. Some of the weakest of them gives only a red or faint purple tincture, and the strongest only a deep purple: but I never saw or heard of any chalybeat water, but this, either in
Scotland.

Scotland or elsewhere, that afforded an intense black and inky colour with galls. From which, I think we may venture to conclude, that the water of this Spaw contains a far larger proportion of iron than most, or perhaps than any, other chalybeate water hitherto discovered: and for this reason, I dare say, it will likewise be so much the more preferable to most others in medicinal virtues; which has indeed already appeared by many surprising cures it has performed, and which, I am persuaded, will more fully appear, when its medicinal effects shall be better known.

There must needs be a very great quantity of iron in this water, when it yields as deep a colour with galls as a strong solution of sal Martis. I was indeed at first apprehensive, that this perhaps might not be owing so much to a large and uncommon proportion of chalybeate parts, as to the commixture of alum, which I judged to be in the water. But we see the contrary appears by these trials: for alum of itself affords no tincture with astringents, and, instead of rendering a solution of sal Martis with galls of a more intense colour, rather makes it lighter and more diluted.

We see here, that the ferrugineous matter is intirely separated from the water by an infusion of galls. The like also happens by elixation; after which it is almost deprived of its tinging quality. Yet other chalybeate waters lose this quality by a much less degree of heat.

As there is an ochreous earth separated from all steel waters, when exposed to the air, which subsides

to the bottom, and a metalline scum or cremor, which swim on their surface; we shall next consider the appearances, which they make in this water.

Exp. 8. A solution of saccharum Saturni being dropt into common spring-water, left the upper parts of the water clear and colourless, but formed a lactescency towards the bottom. The same solution being added to the mineral water, soon turned it of a turbid yellow colour, which afterwards subsided, and formed a deep yellow cloud in the bottom of the glass; and below this yellow sediment there adhered to the bottom of the glass a whitish substance, which I took to be the metalline parts of the saccharum Saturni separated from the purer parts of the salt, which were still suspended in the water, and which made it of a muddy whitish colour.

9. Forty drops of oleum tartari per deliquium being added to an ounce of the water, made it of an uniform light yellow colour; but in an hour afterwards there were many small yellow terrene *nubeculae* formed in it. These the next day were more conspicuous, being thoroughly separated from the water, and precipitated to the bottom, leaving the water quite clear, as it was before the mixture. A small quantity of this limpid water being taken, it would afford no tincture with galls. It was then all poured off, except so much in the bottom of the glass as contained the above-mentioned clouds: to this there were some galls added, which in half an hour turned these clouds from a light yellow to a deep red colour, but did not change the colour of the water, in which they swam.

10. Immediately after the affusion of ol. tart. p. d. to the water, galls were added to the mixture, which tinged it of a deep and bright red colour. After standing for some time, there were red clouds precipitated to the bottom, and the water continued of a dusky opaque red colour.

11. There is a small brook, formerly mentioned, which runs near by these springs; into which the water, that flows from them, is discharged. I observed the stones and channel of this brook all tinged with ochre of a deep yellow colour, so far up as the water of these springs flowed into it; but the channel, which the mineral water ran over before it was mixed with the water of the brook, was very little or nothing discoloured with ochre. As I conjectured what this was owing to, I afterwards took two equal quantities of the mineral water, into one of which I put an equal quantity of common water. In two hours the mixture became less transparent, and appeared yellowish, while the simple mineral water retained its clearness. Next day there was much ochre separated from the mixture, which subsided to the bottom of the glass: but the unmixed mineral water remained still clear and colourless, as at first.

All chalybeat waters separate their ochrous parts, when exposed some time to the air; but this separation is made sooner by the commixture of several kinds of salts. Thus we see the ochre in this water is immediately separated and precipitated by the solution of saccharum Saturni.

The oil of tartar causes a precipitation of these ferrugineous parts in the same manner. Which parts
must

must be the sole cause, that the water receives a tincture from galls; since, after they are precipitate, it loses that quality, which they notwithstanding retain even after they are separated from the water. This precipitation of the ochrous parts of the water were the only visible effects that I could perceive to follow from the affusion of the ol. tart. p. d. I remember indeed, when I was at Moffat, I saw the manuscript of Dr. Horsburgh's experiments upon this mineral water; which appeared to be very accurate; and which I understand are since printed, in a volume lately published by the Philosophical Society at Edinburgh. Amongst these I observed one, which I thought so very remarkable, that I particularly adverted to it. It was the effects of the affusion of ol. tart. p. d. to the water, producing in it clouds, or a coagulation of a green or grass-green colour. I think these were the words; and I own I was something surprised at them. A solution of vitriolum Martis, mixed with this alkaline oil, does indeed produce a green coagulum: but I could scarcely think, that this, or any other chalybeate water, contained so large a proportion of that vitriol, as to be sufficient to produce these effects, when I considered, that so many writers, which I had seen, upon this subject, have all failed in their attempts of extracting a conspicuous martial vitriol from such mineral waters. I had tried this experiment upon four or five chalybeate springs in Scotland, and likewise upon the Spa and Pyrmont waters, which had been well preserved; but there never resulted any such effects from the mixture of these with oil of tartar, as are related in the above experiment. All the alteration it produced

duced in these waters was the precipitation of an ochrous earth, but without the least appearance of any green colour. As I looked upon this as a leading experiment in the history of vitriolic waters; as I had often tried it, and as often seen the green coagulum produced with the solution of the factitious vitriol, and never could observe it produced in any of the above water; I began to suspect, that these waters were either not possessed of a vitriolic salt at all, or else, that it was in some respects very different from the factitious vitriol. For these reasons, Dr. Horsburgh's experiment appeared very extraordinary; tho' at the same time I was greatly pleased, that I should have the opportunity of repeating it, and of observing those phenomena in this ferrugineous water, which I had sought for in vain in several others. But when I came to make the trial, I was yet more surpris'd, when I found it misgive, and that the ol. tart. p. d. produced no green colour or coagulum in this mineral water, nor caus'd any other alteration in it, than the separation of a large quantity of ochrous earth of a yellow colour, exactly the same with what I had observed in the other steel waters. This failure made me immediately conclude, that I had somehow or other committed an error in the experiment: and tho' I was pretty sure, that the mineral water, which I had used in it, was quite fresh, yet I could not be so positive as to the oil of tartar, which I suspected to have been long kept. Yet that this could have been the cause of my being so unsuccessful, I could scarcely believe, tho' indeed I could assign no other. I was sorry, that I had not afterwards an opportunity of repeating this experiment with more accuracy,

accuracy, from which I might have expected to reap more success, as it is perhaps one of the most consequence, that can be performed on this mineral water, as it is capable of demonstrating the existence of a substantial vitriolum Martis in it; which is more than has been hitherto done, or perhaps ever will be done, concerning any one of the vast number of chalybeat waters, which have been yet discovered.

When galls are added to the water, at the same time with oil of tartar, instead of its deep blue colour, it affords only a red tincture.

It appears from the 11th experiment, that an addition of common water causes the mineral water to precipitate its ochre; and the reason of this is obvious: for if these ochrous parts be altogether terrene, as they appear to be, and exist in the water unconnected with any other principle, then it must happen, that as these parts are uniformly diffused thro' the water, in which they are suspended as in a menstruum; by the addition of common water, this menstruum being diluted, the cohesion of these terrene parts must be thereby weakened, and their contact destroyed; so that their menstrual equilibrium being thus taken off, they can be no longer supported in the fluid, but must be precipitated by the force of their own gravity.

Exp. 12. When the water was exposed for some days to the air, there was a cremor separated from it of a shining chalybeat colour. This, like other kinds of cremor, takes a considerable time to complete its intire separation from the fluid, out of which it is expelled: for when it was despumated, a new
cremor

cremor always succeeded, until the whole quantity, which the water contained, was exhausted.

13. When this cremor first appeared on the water, it was of a faint blueish colour: but as it increased, it changed into a deeper and more bright shining blue: and, after longer standing, it became blotched with various colours, as red, orange, yellow, green, blue, purple, and violet.

14. A quantity of the water being put in a gentle heat, this cremor was quickly separated from it, and appeared on the surface of the water. A like quantity of the water, with its cremor already upon its surface, was put over a gentle heat, which by degrees broke the cremor into very small parts; but whether they did evaporate, or precipitate in the water, I could not be certain. But, by other trials, this cremor was found to have a great degree of fixity, bearing a considerable heat without avolation; yet not without the appearance of some of its parts flying off, altho' most of them were fixed; because what remained lost its fine colours, and was changed into a shining chalybeat colour.

15. The water of the lower spring afforded a much less quantity of the cremor, than the water of the upper spring. It took also a longer time to separate, was of a blueish colour, and had not the vivid colours, which the water of the upper spring shewed.

16. When ol. tart. p. d. and spirit of sal ammoniac were added to the water, it did not separate its cremor.

This cremor, which is separated from the water, is the same with that, which appears on the surface of

a solution of vitriolum Martis, when exposed for some time to the air: and an infusion of iron in common water also emits a cremor of the same kind. I remember, as I was once carefully observing a large glass full of a chalybeate water, which contained much of this cremor; soon after it was exposed to the air, I observed a tenuous blueish vapour rising in the parts of the water next the surface, which very much diminished its transparency; and by degrees this vapour was emitted by the lowest parts of the water: but as the cremor increased on its surface, the water became gradually deprived of the blueish tincture, which it received from this halituous body; which was apparently nothing else but the parts of the cremor separating from the water, and ascending upwards. From whence we may conclude, that this cremor consists of the very finest parts of iron attenuated to the highest degree.

It has been the opinion of most naturalists, that these kind of mineral waters do abound in sulphureous parts. This they have conjectured from the foetor, that often attends them. But in what quantity or form these parts exist in the fluid, or by what means they can be rendered conspicuous, has not as yet been sufficiently determined. Yet, I think, we may suspect some of the parts of this cremor to be sulphureous. They are volatile, and, being heated, do fly off from the pure metalline parts, which being more fixed, are thereby left destitute of those vivid colours, which they enjoyed from the sulphureous parts. These are evident marks of sulphur, and are altogether analogous to some other appearances of that mineral. Another observation tending to support this is the want of those vivid colours in the cremor,

cremor, which appears on an infusion of iron ; the reason of which seems to be the loss of the sulphureous parts of the chalybeate minerals by avolation, during the operations of the fire, which they undergo in refining.

It appears from the fifteenth experiment, that the water of these two springs contains a very different proportion of this cremor : and from the last, that it is precipitated along with the ochrous parts, which happens upon the affusion of these alkaline liquors.

The next trials were in quest of alum.

Exp. 17. A quantity of the water being kept for some time in a boiling heat, and after it was cool being filtred quite clear from its ochrous matter, it still retained a subacid and aluminous taste in a very strong degree.

18. To an ounce of common spring-water there was added two gutts of fresh sweet milk. This mixture being shaken, the milk mixed intimately with the water, without any kind of coagulation.

19. The same experiment being made with the mineral water, the milk, upon its affusion, was so curdled, or separated into clouds, that the greatest shaking could not mix or incorporate it with the water.

20. This experiment being also made with a weak solution of alum in spring-water, its effects upon the milk were not in the least different from those of the mineral water.

21. And the same trial being again repeated with

the water, when boiled and filtered from its ochrous parts, the milk was in the same manner coagulated as before elixation.

22. One part of sweet milk being added to four parts of the mineral water, the milk subsided, and formed a cloud in the bottom of the glass, leaving the upper parts of the water clear. This mixture being heartily shaken, the milk mixed so well with the water, that it appeared to be but a very little curdled.

23. When a larger quantity of milk was added to a smaller quantity of water, and even when equal parts of the milk and mineral water were mixed and shaken together, there could be no curdling or coagulation observed.

24. An equal quantity of the water and milk being boiled together, the greatest part of the milk was coagulated into a thick white curd; and the remainder, with the mineral water, turned of a pure white milky colour, which drank like whey, and was very agreeable.

25. Eight gutts of sweet milk being added to four ounces of the water, and the mixture boiled, part of the milk was thereby curdled, and swam upon the top of the water. The ochrous parts of the water were likewise separated, and falling to the bottom, their colour did not appear of a clear yellow, as usual, but was something milky.

All these experiments strongly indicate the existence of alum in this water. It retains its aluminous taste, and coagulates milk, after the chalybeate parts are almost all expelled by elixation. The coagulation.

tion of the milk demonstrates an acidity in the water, and the other appearances shew that acidity to be owing to an aluminous salt.

It appears, that the milk requires a large quantity of the water, to make a sensible coagulation in it: for, in the 22d experiment, one part of the milk being added to four parts of the mineral water, the coagulation was scarcely discernible: and in the 23d, when an equal or larger quantity of milk was added to the water, the coagulation was not at all observable. I have heard it confidently averred, that this mineral water did not at all curdle milk; which, I suppose, has been thro' a mistake in the experiment, in adding too large a proportion of milk to the water: for in this way the coagulation cannot be observed.

I imagined, that when the water was boiled with milk, the mixture would have become of a muddy yellow colour, by the separation of the ochre: but it did not even appear, that the ochre was at all separated from the mixture, as it is from the water when boiled by itself. On the contrary, not only the coagulum, but also the liquor, was of a pure white colour, and of a pleasant taste: and this makes me think it worth the inquiring into, whether or not the water does retain its medical qualities after it is prepared in this manner with milk? For, if it does, such a preparation might certainly be very serviceable in many cases.

These experiments, which we next relate, do not only ascertain the existence of alum in the water with greater certainty, but also, that there is a particular kind of earth conjoined with this salt.

Exp.

Exp. 26. An English quart of the water being kept boiling for a quarter of an hour, it turned thick, muddy, and yellow, by the separation of its ochrous parts; and, being set to cool in a clean bowl, the next day all the ochre was subsided to the bottom, from which the water was carefully filtered: whereby it became almost as clear and limpid as before the elixation, retaining a sharp aluminous taste, but was deprived of the strong ferrugineous taste, which it had at first. This water was again boiled; by which means it was again turned a little yellow, by the separation of some more ochre. It was therefore again filtered, and rendered clear, and its aluminous taste was stronger than before. After this filtration, the water was evaporated in a sand-heat to about a sixteenth part of the original quantity, and then it tasted like a strong solution of alum joined with a small degree of a chalybeate taste. And this being totally evaporated in a glass, there adhered upon its sides a pure white salt; and a larger quantity of the same salt remained in the bottom of the glass, which was not so white, but more impure than the former, and of a brown colour.

27. This salt, thus procured from the water, being mixed with distilled vinegar and spirit of vitriol, there was not the least effervescence produced.

28. Some of the brown-coloured salt being put upon a red-hot iron, it did neither sparkle nor decrepitate; but was turned into a blackish cineritious substance, which in a short time became a white calx. And tho' some of the salt was put upon the iron finely powdered, yet it concreted, and run together in a cinder, whose cohesion was afterwards destroyed when calcined by a further degree of heat.

29. As I was accidentally deprived of the opportunity of obtaining the crystals of this salt, which would have been the best means of knowing to what species it was to be referred ; I dissolved the whole mass in a small quantity of spring-water, and, by filtrating this solution, I obtained a large proportion of fine earth of a brown colour.

30. This solution of the salt afforded a deep blue tincture with galls.

31. The same solution, being mixed with syrup of violets, became of a reddish colour.

32. Saccharum Saturni being added to the solution, precipitated a thick lactescent cloud.

33. Ol. tart. p. d. being also added to this solution, it caused no visible effervescence, yet raised some bubbles of air, and caused a coagulation of many small brown terrene nubeculæ in the water ; which, after standing some time, subsided to the bottom, and left the water clear.

These experiments do plainly evince, that this water contains an aluminous salt, conjoined with a fine terrene substance, which is probably a part of the matrix, from whence the salt has been formed.

This salt gives no signs of any alkaline principle ; but, on the contrary, of an acidity, as its solution reddens with syrup of violets.

With this salt there are also intimately conjoined some very subtil chalybeate parts, which are not separable from it by elixation or evaporation.

Alum is distinguishable from all other mineral salts, by liquifying and bubbling upon a red-hot iron, and turning into a white calx. But this could not be well expected from this aluminous salt, which we had

had extracted from the water, because it was extremely foul, by being combined with so large a proportion of earth; which earthy parts were the occasion of turning the salt of a blackish colour upon the iron. However, we see it turns white by a further degree of heat. But if the salt had been dissolved, filtrated, and crystallized, till it had been purified and freed from this terrene matter, it would then certainly have had the same appearance upon the red-hot iron, as a pure aluminous salt. Again, as it is peculiar to an aluminous salt to liquify in some degree with fire, so we see, that this was evidently the case of this salt. Its eliquation indeed could not be so remarkable, as in pure alum, because of its being mixed with so much earth; but that it did liquify in some degree is plain, because the whole mass of salt and earth, even when reduced to a powder, ran all together like a cinder.

The experiment upon the solution of this salt with ol. tart. p. d. is also a further proof of what we have already asserted: for tho' there was no visible effervescence, yet the bubbles of air shew, that there was an intestine conflict of the oil with the acid principle in the solution; which being absorbed by the alkali, the earth was precipitated, to which it formerly adhered.

The two next experiments were made in order to discover, whether an acid or alkaline principle prevailed in the water.

Exp. 34. Forty gutts of the syrup of violets being added to an ounce of the water, the mixture became of a bright sea-green colour.

35. A quantity of the water being kept boiling for five minutes, and afterwards allowed to stand till it became clear, was carefully filtrated from its ochrous sediment: after which, upon its mixture with syrup of violets, it turned of a faint reddish colour.

From these experiments we infer, that this mineral water contains both an alkaline and an acid principle; the former consisting of the ochrous and ferrugineous parts, which are separated from the water by elixation; and the latter of the aluminous salt, which remains in the water after elixation.

The following trials were made in order to know what effects are produced in the water by being exposed to the air; and in what respects the waters of the two springs differed from each other.

Exp. 37. An English quart of the water of each of the springs being fully exposed to the air in two China bowls, the next day that of the under spring was neither altered in its taste, colour, or transparency, nor in any other shape whatever; but that of the upper spring appeared of a yellowish colour, altho' it was clear and transparent as the other.

On the second day the taste of the waters scarcely appeared to be any way diminished. No sensible change could be observed in the lower water; but the upper water was become more yellow than it was the day before, yet without any loss of its transparency. They both tinged of a deep blue colour with galls; which tinctures appeared equally deep

and strong, as they did before the waters were exposed to the air.

The third day the lower water appeared clear and colourless as before, only its surface was covered with a few small spots of cremor. The upper water appeared more yellowish than formerly, and its surface was almost wholly covered over with the cremor. They both afforded a tincture with galls, which was not sensibly different from what they gave before their exposure.

On the fourteenth day the water of the under well had precipitated a yellow ochrous sediment, but the other water a more considerable quantity. A large quantity of cremor continued also to swim upon the surface of the upper water, but there was very little separated from the water of the under well. Both waters being now tried with galls, instead of the deep blue colour, which they did formerly exhibit, they now became only of a deep purple colour.

On the twentieth day the visible appearance of both waters was the same as when last observed.

On the thirty-eighth day they both afforded as deep a purple colour with galls, as they did three weeks before ; and during that time also they had not precipitated any more of their ochrous parts, nor suffered any other sensible alteration.

The water of the upper well being filtrated from all the ferrugineous matter, which it had separated during these thirty-eight days, was rendered almost as limpid and clear as when newly taken from the well : but, being boiled for some time, it became of a turbid yellow colour ; and being allowed to stand,
it

it again precipitated abundance of an ochrous sediment ; and being filtrated, and mixed with galls, it received a faint purple colour of a blueish hue.

38. A bottle of the water of each of these springs, being carefully sealed, carried to Moffat, and kept for two months, suffered not the least alteration during that time, but was as fresh as when immediately taken from the fountain. And I am informed, that after it is carried to Edinburgh, and to places at a greater distance, it will keep a much longer time without being any way spoiled.

I believe it will appear from these observations, that this mineral water continues longer intire, and particularly that it retains the quality of tinging with galls longer, than most others of the chalybeat kind : at least, of a great number, which I have seen described, I do not remember one, that retains it near so long, when exposed to the open air. Many of them lose this quality intirely in a few hours ; and it is greatly impaired in the same time, even in those which retain it longest. But this water, we see, remains exposed to the open air for days, without almost any alteration. This may perhaps be owing either to the larger proportion of ferrugineous parts, with which it is impregnated ; to their being attenuated to a greater degree ; or to their more perfect commixture with the water, by means of the aluminous salt. The longer time, that any mineral water does remain intire, without any separation of its mineral parts ; or the longer it retains the same form, which it has when newly taken from the spring ; the more perfect is the commixture of these parts with their

fluid vehicle: and I believe, upon that account, will be more effectual for medicinal uses: for which reason, I suppose, these waters may prove a more beneficial medicine, than any others of the ferrugineous kind, whose mineral contents are not so intimately commixed with the aqueous fluid.

As these waters are so long in separating their mineral contents, they appear particularly well adapted for being transported to distant places: for by this quality they are fitted to undergo a long carriage, and to be kept a considerable time, without any diminution of their medicinal virtues. It must also be noticed, that the water of the under well is by much the best of the two for carriage, or for being long kept, as it is longer in separating its mineral contents than the upper one.

From these experiments it is evident, that there is a considerable difference betwixt the waters of the two springs. The upper one contains a much larger quantity of the ochrous earth, and metalline cremor, than the under one; which is the reason, why it yields a deeper colour with galls, as may be observed in the first experiment. I suspected, on the other hand, that the under water contained a greater proportion of alum, than the water of the upper spring; but this I cannot affirm, as I find I had neglected to make the experiment, which would have determined whether it be so or not. Tho' the mineral contents of these two waters be similar, yet, if they be thus mixed in them in different proportions, this must certainly create a difference between them, which deserves to be attended to, as it may be sufficient to disallow of their being used promiscuously, since their medicinal effects may be thereby different.

But

But now, to sum up the evidence, which these experiments, taken all together, do afford, concerning the mineral ingredients of this Spaw; I think they determine, with some degree of certainty, that it contains two different principles of iron, both of which are fixed. The one, which is the ochrous earth, is a true *minera ferri*, and, altho' it be a crude mineral, exists in the water in a very fine and subtile form; the other, which is the cremor or pellicle, whose parts are also extremely attenuated in the water, appears to be iron, not in its mineral, but in its metalline form, and, when thrown up upon the surface of the water, shews itself like an extreme thin *lamina* of that metal. There seems also to be some small proportion of sulphur joined with the metalline cremor. The other mineral ingredient, which enters into the composition of this Spaw, is a considerable proportion of an aluminous salt, which is conjoined with a small quantity of a light brown-coloured earth (probably a part of the matrix whence the salt is formed), and still more intimately connected with some of the chalybeate parts of the water, which are not separable from it either by elixation or evaporation. Whether these be saline or terrestrial, I cannot determine.

Having thus endeavoured to discover, by some plain and simple experiments, the mineral principles, with which this medicinal water is impregnated; I shall now only add some observations, with respect to the origin of steel waters, and particularly of this Spaw, whose origin, I think, is thereby discovered and ascertained in a very obvious manner.

Among

Among several things, that are still deficient in the history of mineral waters, an exact knowledge of their origin seems to be the chief; that is, from what fossils, and in what manner, these waters do acquire the mineral substances, with which they are impregnated. As this happens in the bowels of the earth, and is therefore far removed from our view, it is not surprising, that there has been so little discovered concerning it; tho' indeed there have been many elaborate hypotheses framed in order to account for it.

The writers on mineral waters have been of very different and opposite opinions concerning their origin. They have disagreed widely amongst themselves; and I very much suspect, that the accounts, which most of them give of this matter, are not agreeable to truth: particularly with respect to chalybeat waters, I have seen none, who have given a satisfactory account of their origin. They have all agreed, that iron, or the vitriol of that metal, does exist in mineral waters; but they have never yet agreed, how they came to exist in them, or in what manner mineral waters come to be imbued with these fossils.

Some of the more ancient writers cannot comprehend, how simple water should be intimately impregnated with so many different kinds of minerals, except by the means of some powerful agent. And as they thought nothing more proper for communicating and mixing mineral substances with water, than violent heat, they therefore termed all mineral waters, of whatever kind, by the name of *thermæ*. They saw some spring from the earth extremely hot, others moderately hot, others tepid, others excessively cold:

cold : they concluded from this, that all such various degrees of heat in these waters were owing either to the different degree of subterranean fire, which they had undergone ; or else to the great distance, which some of them had run in the earth, after they had been sufficiently heated. They therefore maintained, that those waters particularly termed *acidulæ* (the greatest part of which are impregnated with iron), or those, which, tho' intensely cold, contained a large proportion of mineral matter, had in some part of the earth been impregnated with it, by means of an intense heat, which they had been gradually deprived of by a long passage thro' the colder parts of the earth.

Some naturalists again, of a later date, having exploded the former notion as chimerical, have thought, that a vapour rising from vitriolic minerals, and mixed with the neighbouring streams of water in the bowels of the earth, has imbued them with some of the parts, and with the properties, of vitriol.

Others are of opinion, that the exhalations of vitriolic minerals, passing thro' the cavities of the earth, are there condensed by the subterraneous cold into a limpid fluid, containing the very finest parts of that mineral salt : which fluid, mixing with the præterlabent streams of water, and issuing out of the earth with them, produce those mineral springs called vitriolic.

The last opinion I shall mention on this subject, and which indeed appears the most plausible, is of those, who think, that the iron is corroded and dissolved in these waters by means of an acid : for, as they imagine simple water incapable of doing this, they

they suppose, that it is first imbued with an acid in the bowels of the earth; and then, by the corrosion of the chalybeate minerals, thro' which it runs, it comes to be impregnated with them. I once received this opinion, as the most probable I could then observe, concerning the origin of these springs: yet not as being satisfactory; for there are many objections against it, which it is difficult either to elude or to answer.

The supposition of an acidity in ferrugineous waters, I thought but ill confirmed, because, upon trial, they discover no vestiges of it, but rather appear to be alkaline. Besides, in considering the causes of mineral waters, it seems more probable, that whatever minerals they contain, they must be such, as can be received or extracted by common water in its passage thro' the earth, by solution, abrasion, or the like simple operations; and in this way alone I think we may come to account not only for the commixtion of the saline and terrene minerals, which are found in medical waters, but likewise of those, that are metalline or sulphureous; for which simple water, at first sight, may perhaps seem to be an insufficient solvent.

It was this notion, that first induced me to make trial upon various mineral and metallic bodies, in order to know how far they could communicate their virtues to common water by infusion. I thought this might throw some light upon the origin of mineral waters: yet, tho' I made a great many experiments of this sort, and particularly upon several kinds of native chalybeate minerals, I was as little satisfied concerning their origin as before. I at length, however,
met,

met, by accident, with what I had inquired after with so little success.

As I happened to be at a gentleman's house near Edinburgh, in whose estate there was a great deal of coal, and who was at that time working a level or adit, in order to drain off the water, I observed, that the current of water, which flowed from this level, separated a great quantity of ochre, and, emptying itself into a river soon after it came from the entry of the level, tinged all the stones and the channel of the river, for a good way, of an ochrous colour. The taste of this water was exactly like that of a common steel Spaw; and it afforded a purple colour with galls *. As I knew, that this water flowed off a great body of coal, I often infused that fossil, taken from the pits near this level, in common water; but the infusions never yielded any tincture with galls. I tried in the same way another mineral, that the miners call *blaes*; which is a cliffery stratum of a blueish colour, that often lies both above and below the coal: also another fossil of a brown colour, which is very ponderous, and is called by the miners *dogger*; a thin seam of which often lies in the midst of the coal. However, neither of these would afford an infusion, that would tinge with galls. At last I got another mineral out of these coal-pits, which is sometimes found amongst the coal, but is not so frequent as any of the former; and this fully

* Within two miles of this place there is a steel Spaw of good repute for the performance of several extraordinary cures, which gives the same tincture with galls, and appears in every respect to be the same with the water, that flows from this level.

answered my expectation. It is found either in round or broad pieces, is exceeding ponderous, and of a shining yellow colour, and is called by the miners *brass lumps*. When I infused this mineral for a short time in common water, it communicated to it all the properties of a steel Spaw; its taste was exactly the same; and it received a tincture from galls, which was of a more diluted or intense purple, according to the proportion of the mineral added to the water, or to the time of the infusion. This simple experiment does therefore clearly discover to us the origin of steel waters, and the manner, in which they are impregnated with their mineral contents in the bowels of the earth.

This observation, which I had made concerning the origin of steel waters, led me, when I first visited Hartfell-Spaw, to inquire into the adjacent fossils: which was the more easily done, as the strata of the earth about the well, for a considerable depth, are exposed to view. After some search among these, I found a stratum of cliffery rock, about three or four feet thick, of a grey colour, and, I think, about twenty paces from the spring. In some of the hollow places of this rock, where the rain and wind did not reach, I observed a white saline efflorescence on its surface, which when I had taken off and tasted, I concluded, from its styptic and chalybeate taste, that it was a native vitriolum Martis, notwithstanding its white colour; but I found it, upon trial, to be alum, having some fine attenuated parts of iron conjoined with it, and the same salt with that contained in the Spaw water.

Having taken some pieces of this rock, which were quite free from the saline effervescence, and infused them in common spring-water for some hours, this water did thereby acquire the true taste of the Hartfell-Spaw. It likewise in the same manner received a deep blue tincture with galls, and contained all the other qualities of that mineral water, without the least difference, that I could observe: which, I think, ascertains the true origin of this mineral spring in the most obvious and undeniable manner.

I am persuaded, that this plain and easy method of investigating the origin of mineral springs is not only superior to the most learned discussions and elaborate theories, but will be found to be the surest, yea, the only way of extending and compleating our knowlege concerning them. As I have not yet had the opportunity of making the experiments, which I designed, upon the two fossils, that we find to be the cause of the above mineral waters, and which will be necessary to elucidate and compleat these observations, which we have made concerning their origin; I shall now only add one thing, and recommend it to the observation of others: “ *Whether or not, from such a knowlege of the origin of mineral waters, we may not acquire artificial ones of as great, or perhaps of greater, medicinal use, than those, which are naturally produced?* ”

XVIII. *An Account of the State of the Thermometer at the Hague on the 9th of January 1757. Extracted from a Letter of Mr. Abraham Trembley, F.R.S. to Tho. Birch, D. D. Secret. R. S.*

Hague, Febr. 15. 1757.

Read Mar. 3, 1757. **I** Carefully observed the thermometer during the cold days, which we have had this winter. I made use of the same thermometer, with which I made my observations in 1740, and for that purpose fixed it in the same place, where it was that year, *viz.* in a window directly exposed to the north, and open to a large square. In 1740 I saw Fahrenheit's thermometer at two degrees below 0. This year, on the 9th day of January in the morning, it was at three degrees above 0; that is, only five degrees higher than in 1740.

XIX. *Experimental Examination of Platina.*
By William Lewis, M. B. F. R. S.

P A P E R V.

Read Mar. 17, 1757. **T**HE account of this extraordinary mineral, formerly read to this illustrious Society, and honoured with their approbation, being since published in the *Philosophical Transactions,*

actions, renders any recapitulation of the discoveries hitherto made unnecessary.

The near and remarkable relation betwixt platina and gold, not only in point of gravity, but in many less obvious properties, hitherto supposed to belong to gold alone; and their as manifest disagreement in others, particularly colour, ductility, and fusibility; induced me to examine, what effects they might have in combination with one another in different proportions; and whether there is reason to credit the report of great frauds having been committed by mixing them together; how far such abuses are practicable; and, what is of more importance, the means by which they are discoverable.

Experiments of the Mixture of Platina and Gold.

EXPERIMENT I.

I. Twelve carats* of fine gold, and the same quantity of the purer grains of platina, were urged in a blast-furnace, for near an hour, with a fire so strong, that a slip of Windsor brick, with which the crucible was covered, tho' defended by a thin coating of pure white clay, had begun to melt. Upon breaking the vessel, the metal was found in one smooth lump or bead; which, after being nealed by the flame of a lamp, and boiled in alum-water, appeared,

* The proportions were adjusted according to the carat weights, as it is by these, that the fineness of gold is usually expressed. A carat is the twenty-fourth part of the whole compound: thus gold of so many carats is a composition, of which so many twenty-fourths are fine gold, and the rest an inferior metal.

both in the mass, and upon the touchstone, of a pale bell-metal colour, without any resemblance to gold. It bore several strokes, and stretched considerably under the hammer, before it begun to crack about the edges. On viewing the fracture with a magnifying glass, the gold and platina appeared unequally mixed; and several small particles of the latter were seen distinct: nor was the mixture intirely uniform after it had again and again been returned to the fire, and suffered many hours of strong fusion.

2. Eighteen carats of gold and six of platina ($=3:1$) were melted together as the foregoing, in an intense fire continued about an hour. The bead, nealed and boiled, was less pale-coloured than the former, but had nothing of the colour of gold. It forged tolerably well, like coarse gold. To the naked eye it appeared uniform; but a good magnifier discovered in this, as well as in the other, some inequality of mixture, notwithstanding the fusion was two or three times repeated, with the strongest degrees of heat we were capable of exciting by large bellows.

3. Twenty carats of gold and four of platina ($=5:1$) were kept in strong fusion for above an hour and a half. These united into an equal mass, in which no granule of platina, or dissimilarity of parts, could be distinguished. The colour was still so dull and pale, that the compound could scarcely be judged by the eye to contain any gold. It hammered well into a pretty thin plate; but we could not draw it into wire of any considerable fineness.

4. Twenty-two carats of gold were melted in the same manner with two of platina ($=11:1$) the
7
same

same that standard gold contains of alloy. The mixture was uniform, of a tolerable golden colour, but easily distinguishable from that of standard gold by a dingy bell-metal cast. It worked well, was forged into a thin plate without cracking, and drawn into moderately fine wire.

5. Twenty-two carats and a half of gold, and one and a half of platina ($=15:1$), melted into an uniform mass, which, after the usual nealing and boiling, proved somewhat tougher than the preceding, and of a better colour.

6. Twenty-three carats of gold were melted with one of platina; which is nearly half the proportion, that standard gold contains of alloy. The compound worked extremely well, but was distinguishable from gold by a manifest dinginess, which it retained after repeated forgings, fusions, nealings, and boilings.

7. Twenty-three carats and one-fourth of gold, and three-fourths of a carat of platina ($=31:1$), formed an equal mixture, very malleable, ductile like the three foregoing whilst hot as well as cold, but not intirely free from their peculiar dingy colour.

8. A mixture of twenty-three carats and a half of gold, with half a carat of platina ($=47:1$), was very soft and flexible, of a good colour, without any thing of the disagreeable cast, by which all the foregoing compositions were readily distinguishable, in the mass as well as on the touchstone, from fine or standard gold.

9. A mixture of twenty-three carats and three-fourths of gold, with one-fourth of a carat of platina ($=95:1$), could not be distinguished by the eye or hammer from the fine gold itself.

In all these processes, even where the proportion of platina was small, the fusion was performed by a vehement fire, that the mineral might be the more intimately dissolved, and equally diffused thro' the gold. The necessity of this precaution appeared from an experiment formerly related; in which one of platina having been melted with four of gold, the button appeared not much paler than standard gold with silver alloy. On a second fusion it lost its yellow colour, which had at first been only external, from an imperfect mixture, great part of the platina being concealed in the internal part of the mass, and covered as it were by a golden coat.

The crucibles were rubbed on the inside with chalk, to prevent any particles of the metal from lodging in their cavities. A little borax was employed in each as a flux; with the addition of nitre, by which the colour of gold is somewhat heightened. On remelting some of the mixtures with sundry other additions, powdered charcoal seemed to improve the colour most.

EXPERIMENT II.

The preceding compositions, after being gently hammered and boiled, were weighed hydrostatically with great care, by a very tender balance, in distilled water, wherein the gravity of standard gold turned out 17.788.

All the mixtures proved heavier than standard gold. Their gravities were nearer to the medium of the gravities of the ingredients, than those of the compositions of platina with any of the other metals
formerly

formerly given an account of; none falling considerably short of the mean gravity, and some rather exceeding it.

		Gravity.		Difference.	
		By Experiment.	By Calculation.		
Platina		17.000			
Platina	Gold 1	18.140	18.142	0.02	} Diminution.
Platina	Gold 3	18.613	18.714	0.101	
Platina	Gold 5	18.812	18.904	0.092	
Platina	Gold 11	18.835	19.094	0.259	
Platina	Gold 15	18.918	19.142	0.224	
Platina	Gold 23	19.089	19.189	0.100	} Increase.
Platina	Gold 31	19.128	19.213	0.085	
Platina	Gold 47	19.262	19.237	0.025	
Platina	Gold 95	19.273	19.261	0.012	
Gold		19.285			

EXPERIMENT III.

As a mixture of platina with an equal quantity of gold has been reported to be specifically heavier than gold itself, but turned out otherwise in the above experiments; some further trials were made on that head.

1. Instead of the crude mineral, whose gravity is but 17, we took platina, that had been cupelled with lead, one of the neatest of the buttons formerly mentioned, which, tho' retaining a portion of the lead, was nearly as ponderous as fine gold, *viz.* 19.240. This was melted with equal its weight of the gold, in a strong fire, and continued in fusion for about an hour: the mass proved spongy, and very light. We

remelted it several times with vehement degrees of fire, suffering it to cool leisurely in the crucible; and, in order to separate as much as possible of the lead, to which its sponginess seemed owing, boiled it in aqua-fortis, and repeatedly injected corrosive sublimate upon it during fusion: the mass, nevertheless, still turned out cavernulous and brittle, and specifically lighter than either the gold or platina by themselves.

2. I likewise endeavoured to combine platina with small proportions of gold. By vehemence of fire, it was made to unite, tho' not perfectly, with half its weight and less: but the mixtures were extremely spongy and brittle; in specific gravity one scarce 16, another less than 15.

3. As a cast metalline body from the Spanish West Indies, of which some account will be given hereafter, appears to have been confounded with the mineral platina, this also was melted with an equal quantity of gold. They united with great ease, by a moderate fire, into an uniform compound, tolerably compact, but whose specific gravity was only $16\frac{1}{2}$; which is nearly the mean gravity of the two ingredients.

EXPERIMENT IV.

As a small portion of copper somewhat heightens the colour of pale gold, platina was melted with eight times its weight of standard gold made with copper alloy. The fusion was performed, as in the preceding experiments, in a close crucible, with a strong fire, but without any flux, and continued for about an hour. The metal appeared covered with a
black

black scurf, and had lost about $\frac{1}{200}$. It was much duller coloured, harder to the hammer, and cracked sooner about the edges, than mixtures of fine gold with a larger quantity of platina. By repeated fusion, and frequent nealing, it became a little softer and tougher, so as to be drawn into pretty fine wire; but the colour was still exceeding dull, more resembling that of bad copper than of gold.

The specific gravity of this compound was 17.915; a little less than the medium of the three ingredients unmixed, and a little greater than the mean gravity resulting from the platina by itself, and the copper and gold mixed; for copper, in the standard proportion, appears to diminish the gravity of gold more than it ought to do according to calculation.

From the foregoing experiments it appears, that platina is miscible with gold, in certain proportions, without injuring either its colour or ductility, or occasioning any considerable alteration in the gravity: experiments related in former papers have shewn, that it stands aqua-fortis, and the other trials by which the purity of gold is estimated. It is to be hoped, that the abuses manifestly practicable by this mineral have hitherto been but rarely made use of. To guard against them is the object of this paper; to detect them. of the next.

XX. *Experimental Examination of Platina.*

By William Lewis, M. B. F. R. S.

P A P E R VI.

*Experiments of distinguishing and purifying Gold mixed with Platina.*1. *By Amalgamation with Mercury.*

Read Mar. 31, 1757. **I**N an experiment related in the fourth paper, an amalgam of one part of platina and two of gold with a suitable quantity of mercury, having been triturated with water for a considerable time, and occasionally washed over, the platina was gradually thrown out, and the gold retained by the quicksilver.

Repetitions of this experiment have shewn, that tho' the separation succeeds in some cases, it does not perfectly in all: that if there is any particle of the platina imperfectly dissolved in the gold (which will generally be the case, unless the quantity of gold is three or four times greater than that of the platina), this part will be retained, after long trituration, undissolved by the mercury, uncomminuted by the pestle, and too ponderous to be washed off in its gross form. A variety of mixtures of platina and gold were treated in the manner above described; and the gold, recovered from the amalgams, submitted to further examinations. Where the proportion of platina was large, the microscope almost always discovered still some granules of it on the fracture of the

the ingot: where the proportion was small, the recovered gold was frequently, but not constantly, found to be pure.

From these experiments it appears, that mercury has a greater affinity with gold than platina, and that platina is capable of being totally separated by elutriation; but that the process is too vague and undetermined to be applicable in the way of assay, as we have no mark of the precise time for discontinuing it, and as we can never be certain, without making another assay, whether the whole of the platina is separated or not. As a preparatory examination, where the quantities of platina and gold to be separated are large, it is nevertheless of good use, as greatest part of the platina may by this means be washed over with little trouble, and the gold brought into a less compass, so as to be commodiously submitted to a perfect purification by the means hereafter pointed out. This process has a similar effect on platina and gold to that of stamping and washing on metallic ores; which could not be reduced into pure metal in the furnace to advantage, without the previous separation of great part of the earthy and stony matter by water.

2. *By Precipitation with Alkalies.*

Gold is precipitated totally by fixed alkaline salts, but platina only in part. When solutions of the two metals are mixed together, so much of the platina remains suspended, after saturation with the alkali, as to be readily distinguishable by the yellow colour, which it communicates to the liquor. It has been
objected,

objected, that tho' the platina was discoverable, when thus mingled superficially with the gold, it may nevertheless, when combined more intimately by fusion, elude this method of trial.

1. Mixtures of gold with small proportions of platina were therefore kept in fusion, by a very strong fire, for several hours, and afterwards dissolved in aqua-regis. The solutions being diluted with water, and a pure fixed alkaline salt gradually added, so long as any effervescence or precipitation ensued, the liquors remained manifestly coloured, tho' apparently paler than when the two metals had been dissolved by themselves.

2. A more convincing proof, that part of the platina remains suspended, after the precipitation of the gold, was obtained, by putting into the filtered liquors some plates of pure tin, which presently contracted an olive hue, and threw down a large quantity of a brownish precipitate, as from the common solutions of the crude mineral. It was observable, that the tin plates were often sensibly acted on, even whilst the liquor was overcharged with alkali.

3. It has been further suggested, and with great appearance of probability, that as a part of platina is precipitated as well as gold by alkaline salts, if only this part be mixed with gold, it will be thrown down by them again upon dissolving the compound. To determine this point, a precipitate of platina made by fixed alkali was melted with thrice its weight of fine gold, and kept in strong fusion for above an hour: they united more easily than go'd does with so large a proportion of the crude mineral, and formed a smooth neat bead, which hammered well
into,

into a pretty thin plate before it cracked, and appeared internally uniform and equal. This compound being dissolved in aqua regia, and a fixed alkaline salt added by degrees till the acid was more than saturated, the liquor became indeed pale; but tin plates put into it quickly discovered, that it held a very considerable quantity of platina. It appears therefore a constant property of this mineral to remain partially dissolved in the neutralised liquor; and that minute proportions of it, mixed with gold, are by this means distinguishable.

4. Many other experiments were made of the precipitations and precipitates of gold and platina, by alkalies both of the fixed and volatile kind. The most remarkable effects were, that volatile alkalies, added to both solutions in quantity just sufficient to saturate the acid, precipitated gold intirely, but platina only in part, so much of it remaining suspended as to give the same colour to the liquor as when fixed alkalies were made use of: that, on adding a larger quantity of the spirit after the precipitation of the gold, the liquor became yellow, a part of the metal being taken up again; and that the platina was more copiously redissolved, the liquor becoming of a deep brownish red: that the washed precipitates of both metals, whether made by volatile or fixed alkalies, proved soluble, by moderate digestion, in spirit of salt; those of platina much more difficultly and sparingly than those of gold.

3. *By inflammable Liquors.*

1. ~~Inflammable~~ spirits, which revive gold from its solutions in form of yellow films, have no such effect

on solutions of platina. This experiment affords not only a criterion for distinguishing with certainty whether gold has been debased by platina, but likewise an infallible means of recovering it perfectly pure from any admixture of that mineral. If the compound be dissolved in aqua-regis, the solution mingled with twice its quantity or more of the spirit, and the mixture suffered to stand for some days in a glass slightly covered; the whole of the gold arises to the surface, leaving the whole of the platina dissolved. The golden pellicles may be collected, by pouring the matter into a filter just large enough to contain it. The dissolved platina passes thro', leaving the gold upon the paper, which is to be washed with fresh parcels of water till the liquor runs colourless. The paper is then to be carefully squeezed together, and burnt in a crucible previously lined with vitrified borax: when fully sunk down, a little fresh borax is to be injected, and the fire raised to melt the gold. The use of lining the crucible with borax is to prevent any molecularæ of the gold from lodging in its cavities.—This process is attended with one inconvenience, the slowness of the separation of the gold from the solution: this may be in some measure expedited by employing a spirit, which has been distilled from vegetables, that give over an essential oil.

2. As essential oils take up gold from aqua-regis, and keep it dissolved for a time upon the surface of the acid; a pure colourless oil, that of rosemary, was poured into about half its quantity of a solution of platina, the mixture well shaken, and suffered to rest: the oil quickly arose, without taking up any thing from the platina, or receiving any colour: the acid
liquor

liquor underneath remained coloured as at first. Compositions of platina and gold being dissolved in aqua-regis, and treated in the same manner, the whole of the gold was imbibed by the oil, and the whole of the platina remained dissolved in the acid. The oil, loaded with the gold, appeared of a fine yellow colour, and, on standing for a few hours, threw off great part of its contents, in bright yellow films, to the sides of the glass. Sundry other distilled oils were made trial of, with the same event. The gold is easily recovered, by setting the oil on fire; and, when thoroughly burnt out, melting the residuum with borax, as in the preceding experiment. After the separation of the oil employed at first, it may be proper, for the greater security, to add a little more; which, if any part of the gold should happen to have been left in the liquor, will effectually take it up.

3. The experiment was repeated likewise with the subtle fluid, prepared from vinous spirits with the vitriolic acid, called by the chemists æther. The separation succeeded in the same manner as before; the æther receiving nothing from pure platina, but instantly taking up the gold from a mixture of the two. It is observable, that the gold imbibed by this fluid is kept permanently dissolved by it; without separating or reviving, as it does from the common essential oils and vinous spirits.

4. The liquors remaining in these experiments, after the extraction of the gold, appear on all trials the same with the common solutions of platina; and readily betray their being impregnated with that mineral by their colour, by the precipitation with

tin, by their yielding a sparkling red precipitate with volatile spirits, &c. A far more minute proportion of platina, mixed with gold, is more distinguishable by these processes, than by those with alkaline salts above-mentioned; these exhibiting the whole of the platina dissolved by itself, those only a part of it.

4. *By metallic Solutions.*

All the metals, which precipitate gold from aqua-regia, have been already shewn to precipitate platina also. As gold is thrown down by some metallic solutions, as well as by the metals in substance, particularly those of mercury and iron, it remains to apply these liquors as precipitants for platina.

1. A saturated solution of mercury in aqua-fortis, which readily and totally threw down gold in its metallic form, being added to a solution of platina, the liquor became immediately turbid, and, on standing for a little time, nearly the whole of the platina fell to the bottom. A solution of mercury in the marine acid, or of corrosive sublimate, likewise precipitated platina, but less perfectly, and with this difference, that the former precipitate was of a greyish brown colour, the latter of a sparkling red.

2. Solutions of iron in the vitriolic acid, or of common green vitriol in water, which totally threw down gold, happily made no change in solutions of platina. Compositions of platina and gold being dissolved in aqua-regis, the solutions diluted with about twice their quantity of water, and a filtered solution of the vitriol gradually added; the mixtures instantly grew turbid, and, on standing, deposited the gold in
form.

form of a purplish grey calx, the whole of the platina remaining dissolved. It appeared, on numerous repetitions of this experiment, that no part of the platina was precipitated along with the gold, nor any of the gold kept suspended with the platina. Where the quantity of the mixt to be assayed was very small, the precipitation was usually performed in a filter, that the gold, which separates in very minute moleculæ, some of which might possibly remain unobserved in the bottom of a glass, might be detained on the paper. The colourless sorts of filtering-paper are preferable for this use to the coloured; as these last may be impregnated with astringent matter, which would extricate some of the ferrugineous part of the vitriol. The vitriol was dissolved in about six times its quantity of water, and a few drops of oil of vitriol added, to prevent the separation of any of its iron in the filter. This solution was put into the filter first, the solution of gold and platina immediately poured into it, the whole stirred together with a clean glass rod, and such part of the liquor, as had run thro' before they had been duly mixed, poured back to the rest. The gold remaining in the filter was washed with fresh parcels of water, the paper cautiously rolled up, and burnt in a crucible, as mentioned in a former experiment.

3. Solutions of the vitriol, recommended by Kunkel and others for precipitating gold of an uncommonly high colour, made no change in the solutions either of gold or platina. The bluish green did indeed precipitate the gold; not as blue vitriols, but by virtue of the ferrugineous matter, of which these kinds largely participate. White vitriol was likewise

made trial of, but without producing any sensible effect in either solution.

4. The experiments with green vitriol were repeated on the solutions of platina and gold made in spirit of salt. The event was the same as with those made in aqua-regis; the gold being constantly precipitated, and the platina remaining dissolved.

REMARKS.

It may be proper to observe, that by the processes here pointed out, the gold is purified from other metallic admixtures at the same time that it is separated from platina; the inflammable spirits reviving, essential oils and æther imbibing, and green vitriol precipitating, gold alone. Care should be had, that the piece of the mixt, taken for examination, be totally dissolved before any trials are made with the solution; the menstruum not acting with equal facility on the two metals, but dissolving the gold more readily than the platina. Where the acid has been dilute, and only a gentle heat applied, great part of the gold has appeared to be taken up before the platina was considerably acted on. Where the filter, with the gold in it, is burnt in the crucible, borax is the most commodious flux: but as this salt gives a sensible paleness to gold, a little nitre may be injected, after the metal has come into fusion, to restore its colour. If the nitre was added at first, whilst the gold continues subtly divided, some particles of the metal would be dissipated during the deflagration, which that salt produces with the coaly remains of the paper.

As the foregoing experiments exhibit platina and gold dissolved in a mineral fluid, which by simple mechanic agitation rejects the one and retains the other, and which discovers this different appetite of union so much the more remarkably, as the two metals have been the more intimately combined : — as they further exhibit platina dissolved in liquors incapable of holding gold suspended, — gold dissolved in liquors incapable of holding platina suspended, — gold totally precipitated by substances, which precipitate no particle of platina, — and gold, when mixed *per minima* with platina, perfectly recovered from it by these means, without increase as well as without diminution : — it follows, that platina is not, as some believe, gold naturally debased by the admixture of some other metallic body, but a metal of a peculiar kind, essentially different from all the others. Before the discriminating characters of platina were discovered, such a notion was highly plausible, and direct experiment seemed to confirm it : a portion of the platina might be separated in the process ; a quantity of gold mixed with the remainder, in order to collect the gold supposed to be contained in it ; the mixture submitted to operations, which gold alone was supposed capable of withstanding ; and the augmentation, which the noble metal still retained, held to be true gold gained from the platina.

The methods of trial above related will, it is presumed, be sufficient to undeceive those, who may have been imposed upon by such appearances, and betrayed into the practice of unintended frauds : to convince them, that all they have gained from platina, after the most laborious attempts to divest it of
its

its imaginary coat, is no other than platina still : and, which is of more extensive utility, to distinguish all the abuses, that may be made with this metal, and restore the gold, so debased, to its original purity and value.

XXI. *An Account of the Temple of Serapis at Pozzuoli in the Kingdom of Naples : In a Letter to John Ward, LL. D. and R. S. Vice-Præs. by the Rev. John Nixon, M. A. F. R. S.*

S I R,

Read Mar. 17.
1757.

BEFORE we enter upon a more particular consideration of this noble piece of antiquity, it may not be improper to premise the general account (and indeed the only one I have met with yet published), which is given of it by Mess. Cochin and Bellicard, in a little (1) treatise printed at Paris in 1755. These gentlemen acquaint us, that in 1749 there were only three pillars of this building visible, and that they were buried half way within the ground : but that soon after, workmen being employed by order of the King of the Two Sicilies to dig at the place, they came to the pedestals of those pillars ; and at length discovered the building to have been a temple, which

(1) *Observations sur les Antiquités d'Herculanèum, &c.* p. 82.

(as it was judged by the principal (2) idol found there, and some other circumstances) was dedicated to Serapis. They tell us further, that many statues and vases of excellent workmanship had been taken out of the ruins; and that the whole temple was extremely magnificent, being built, or cased throughout, with marble, even to the parts appropriated to the meanest offices.

This account, tho' short, is yet sufficient to excite in the curious a desire to be more fully informed, both as to the ancient and modern state of this place. To gratify in some measure that desire is the purpose of the present letter.

In order to form any conjecture concerning the antiquity of the building before us, we must know, that the worship of Serapis, to whom it is supposed to have been consecrated, was not introduced at Rome till towards the end of the republic; and then tolerated in the suburbs only (3). However, at length he was allowed to have temples erected to him within the precincts of the city; chiefly by the authority of Vespasian, who was thought to have restored a blind man (4) to his sight at Alexandria by the aid and direction of this deity. And upon this account he continued to be held in high veneration by Titus and Domitian, the sons of that Emperor, as appears by their (5) stamping his

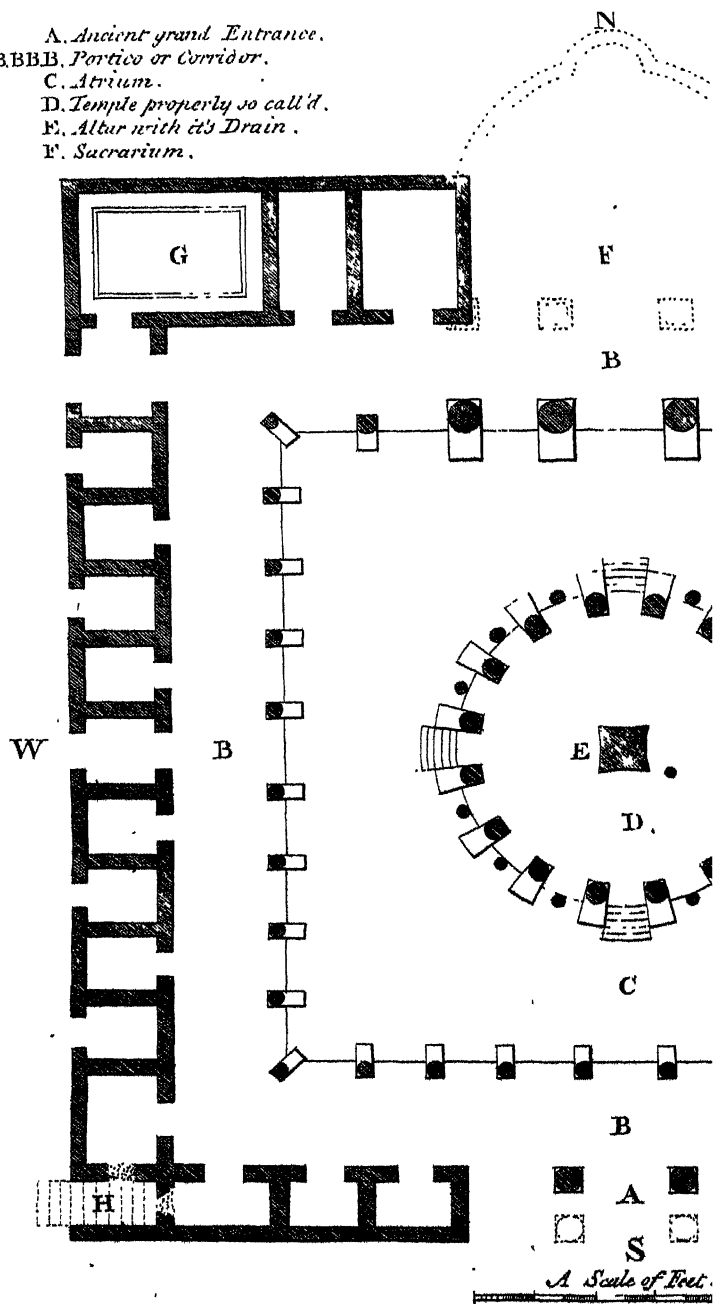
(2) For a more particular account of this statue, now in the palace at Portici, I beg leave to refer you to a paper of mine read before the Royal Society on Feb. 24, last.

(3) Dio. L. xl.

(4) Suet. Vesp. c. 7.

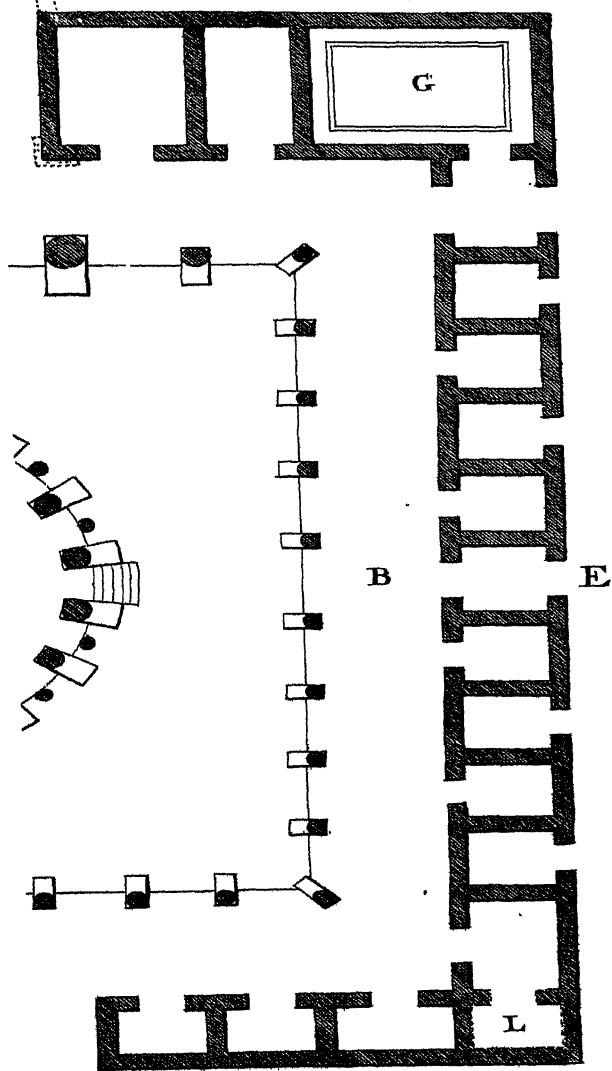
(5) For authorities, see Middleton's *Germana Antiq. Mon.* R. 152.

- A. Ancient grand Entrance.
 B.B.B.B. Portico or Corridor.
 C. Atrium.
 D. Temple properly so call'd.
 E. Altar with it's Drain.
 F. Sacrarium.



G.G. Two large Chambers for washing &c.

H. Modern Entrance over Ruins. This seems to have been formerly such a Chamber, as is exprefs'd in the Drawing at (L)



worshippers by bathing or washing. This last destination is countenanced, with regard to the chamber on the north-west and that on the north-east corner, by the row of stone seats, which still remains on each of the sides of the former. These seats have a gutter, or channel running along at the foot of them on the floor; and are likewise perforated with holes of a proper size, with funnels passing from them below. On these benches probably the persons to be purified placed themselves, that the water might be let out upon them from pipes; or administered in vases or ewers by the attendants, and afterwards be carried off by the passages mentioned above (7).

Nor can a provision for washing or bathing in this temple seem strange to any one, who reflects, how high a rank this mode of purification held among the religious ceremonies of almost all nations of the world. As for the Romans, with whom we are principally concerned in the present inquiry, the subsistence of this usage among them might be abundantly shewn by the testimonies of their writers; and also by the accommodations provided for it in other buildings of the same character with that before us. Some of these still remain within the neighbourhood of Pozzuolo, *viz.* the magnificent temple near the lake of Avernus ascribed to Apollo, which has an apartment adjoining to it indisputably intended for the purpose

(7) Mess. Cochin and Bellicard seem to think this room was intended for another purpose, by their calling the funnels under the holes in the seats of it, *conduits des fossés d'aisance*. Which of the two hypothesis's is to be preferred, I submit to the judgment of the learned; or rather, whether both of them may not be admitted, as in no-wise incompatible the one with the other.

intimated above. For it is furnished with several stone cisterns, whose inward dimensions are proportioned to the ordinary size of an human body; and near them is a spring, out of which the water was taken up, and poured into a basin hollowed out in the side of the wall: from thence it ran along in a groove or channel cut in the ends of the cisterns, to be let out upon the persons bathing in them, according to their pleasure, or as occasion should require. So likewise in the temple of Venus (as it is commonly called) near Baïæ, there is a large chamber containing several stone seats for washing, with little cells contiguous to it for undressing before, and anointing the body and dressing afterwards.

I have but one more particular to add concerning the apartment in the north-west angle of the temple, viz. that, when it was cleared of its rubbish, there was found in a niche in one of its sides a male and female figure naked, and in the most flagrant act of natural lewdness. It is now (as we were informed) in a private room in the palace at Portici, nor can be seen without the King's special permission. In the same place, probably, may stand the statue of a satyr in an unnatural action with a goat, which was found at Herculaneum, and is, they say, of exquisite sculpture, but concealed in the palace above-mentioned with the same strict care as the former.

Having thus viewed the several chambers in the exterior parts of the building, it is requisite (in order to a regular prosecution of our design) to return to the grand entrance. And here, passing thro' the corridor above described, we come to a square court or *atrium* paved with large slabs of white marble streaked

streaked with blue or greyish veins. At the distance of 25 feet further, in the center of the said court, stood the temple properly so called, containing a circular area of 54 feet diameter, and elevated above the level of the pavement, so as to admit an ascent to it of five steps (8), in four different parts answering to the four sides of the corridor. This area is surrounded with sixteen pedestals, on which formerly were columns to support a rotundo or dome. Against each of these columns, on the outside, there seems to have been placed a statue, and, in the intermediate spaces, vases for incense, or lavers for washing, upon low stands on the floor. In the middle of the temple was erected the grand altar, the traces of which still remain, with a sink or drain near it to receive and carry off the blood of the victims, &c.

Northward of the temple, and at the distance of 25 feet, being the same space, that was between it and the corridor at the entrance, was once another stately vestibule or pavilion, supported by four columns four feet and an half in diameter, and of the Corinthian order, as appears by three of them, which still subsist standing in a line with the outer face of the corridor. This pavilion (if we may judge by analogy from what we find in other temples) led to an inner recess or *sacrarium* terminating, probably, in the segment of a circle: but of this we had no certain proof, as the rubbish was not yet removed from this part of the building:

(8) Vitruvius, Lib. iii. cap. 3. *Gradus in fronte ita constituendi sunt, uti sint semper impares: namque cum dextro pede primus gradus ascendatur, item in templo primus erit ponendus.*

I beg leave further to mention a remarkable appearance in some of the columns of this temple, viz. that that part of them, which was lowermost, as well as that, which was nearest the capitals, is well preserved and pretty entire; while part of the intermediate space for two or three feet together is discoloured, as if it had suffered by burning; and also excavated in such a manner (9), as to contain multitudes of little shell-fish, which appear, like the pholades in some stones, almost totally inclosed within their cells, so as not to be got out without breaking. I know no way to account for this so probable, as by supposing, that the lower parts of these columns were secured by the mass of rubbish, that inclosed them, as the uppermost were by their elevated situation, from being perforated either by the corrosive quality of the sea, which (according to tradition) formerly covered the site of this temple; or by the animalcula, which are bred in that element; while the middle parts standing in the water were (perhaps for ages) exposed to the injuries mentioned above. I had no opportunity of taking the height of the uppermost line, where the above-mentioned

(9) The learned Abbate de Venuti, F.R.S. and Antiquary to the Pope at Rome, has lately by letter favoured me with an ingenious account of this phenomenon; tho' he mentions the granite columns only as affected by it. *Cum columnæ, quæ circumbant templum, excavarentur e terrâ, quâ erant partim abruptæ — minutissimæ conchæ, quæ ex testaceorum genere sunt, atque in saxorum rimulis prope mare reperiuntur, ideoque a vulgo Trutti di Mare appellatæ, columnas hæc (i. e. Thebaicas) quam sæpissimè perforaverant, sese componentes, veluti apes in alveari, cum essent sejunctæ integumentis ex ipso lapide subtilissimis. —*

alteration

alteration in the columns ended, from the level of the sea in the bay ; which would have shewn how high the water must have risen formerly above its present mark, to produce the effect ascribed to it on this hypothesis. But, however that may be, the nature of the situation of this place (10) being considered, the innovation supposed to have happened in it will not, I presume, be thought improbable ; especially in a country so plentifully stored with combustible matter in its interior parts, and consequently so liable to changes in its outward form, as this is, and has been for many ages. For an extraordinary instance of this we need go but a little way from this place, *viz.* to Il Monte Nuovo, a hill about four miles in compass, which in 1538 was cast up in one night's time by an eruption, whereby the greatest part of the Lucrine lake was filled up, and the town of Tripergola, with a church, convent, hospital, and other buildings, intirely buried.

I shall conclude with acquainting you, that at the corner of the court of this temple, near the present entrance, there lie some large bases of marble, which (as we were informed) were taken out of the sea at about a mile's distance from Pozzuolo, and are inscribed DVSARI SACRUM. What was the original situation of these monuments, it may not be possible now to ascertain. As for DVSARES mentioned in the inscription upon them, G. Vossius (11)

(upon

(10) Vid. p. 168.

(11) Nec alijs a Διονύσω five sole est Δυσάρης, five Δυσάρης, vel Δευσάρης. Quæ vox (ut suspicor) conflata ex ΔΥΤ Dutz et ΔΥΝ Acato.

(upon the authority of Tertullian, and Stephanus Byzantius) makes him to have been an Arabian deity, the same as Bacchus or Sol according to the Roman theology. That learned man is likewise inclined to think, that the name DVSARES is compounded of two Hebrew words; one of which signifies *joy*, and the other, *the earth*, i. e. mortals, who inhabit it. This etymology properly expresses the genial effects of the sun, which makes glad the heart of man by ripening the fruits of the earth, especially the grape: Whence Virgil calls wine,

Munera lætitiæque Dei. Æn. i. v. 640.

and stiles Bacchus the fabled inventor of it,

— *Bacchus lætitiæ dator.* Ib. v. 738.

I am,

S I R,

With the greatest respect,

Your most obedient humble Servant,

London,
March 14. 1757.

J. Nixon.

Arctz. Quorum prius (*gaudium*) alterum notat (*terram*) ut notet *lætitiæ terræ*, sive mortalium. Nam Liber sive sol lætitiâ implet mortales, maturando fruges, et uvas, unde de vino sic Maro,

Munera lætitiæque Dei. Æn. i.

Dufarem verò esse Arabum numen indicat locus ille Tertulliani in Apolog. c. 24. “Unicuique etiam provinciæ et civitati suus deus est. Ut Syriæ Astartes; ut Arabia Disares.”

Etiam apud Stephanum sive Hermolaum Byzantium — Δυσάρη — σκόπελος ἢ κορυφὴ ὑψηλοτάτη Ἀραβίας. *Εἰρηται δὲ καὶ τῷ Δυσάρη. Θεὸς δὲ οὗτος παρὰ Ἀραβίαν καὶ Δαχαρινὸς τιμώμενος — Vossius de Idololat. L. ii. c. 8.

XXII. *Some Remarks on a Parthian Coin with a Greek and Parthian Legend, never before published. In a Letter from the Rev. John Swinton, M. A. of Christ-Church, Oxon. F. R. S. to the Rev. Thomas Birch, D. D. Secret. R. S.*

Reverend Sir,

Read Mar. 24,
1757.

AS we know little of the reigns of the latter Parthian kings, and less still of their coins; an attempt to strike out even the least new light on either of those heads will not, I persuade myself, prove unacceptable to the Royal Society. In this persuasion, I do myself the honour to send you a few conjectures upon another brass Parthian medal, in my possession, as remarkable, on account of the double legend it contains, as the former, which I endeavoured a little to illustrate about a year ago. This medal, which certainly merits the attention of the curious, is in very good conservation, and approaches near the size of those of the middle Roman brass. It exhibits the head, or effigies, of a Parthian king, with a beard, diadem, and hair formed into such curls as I never yet observed upon any antient coins. Under the effigies, the Greek letters ΒΑCΙΛΕΩΝ ΜΕΓΑC ΜΟΝΕΤΗΡ, which demonstrate the piece to be Parthian, appear; and, on the reverse, a *Victory*, done something after the Roman manner, tho' the workmanship is pretty rude, presents itself to our view, together with a legend in a language
and

and character at this time unknown. The legend consists of ten complete elements, *placed behind the *Victory* above-mentioned; besides which there is one in the field of the medal, being probably the initial letter of the name of the city where the coin was struck. The metal, tho' termed by me brass, discovers something of a composition similar to that of his Grace the Duke of Devonshire's medal of Vologeses III. as described by (1) Sig. Haym. This coin, of which I herewith transmit you a draught most accurately taken (*See* TAB. IV. *Fig.* 1.), having not yet, as I apprehend, been published; you will permit me now to offer a few cursory remarks upon it, drawn up in the shortest and most concise manner possible.

1. The Greek legend, had all of it appeared, would undoubtedly have been either ΒΑCΙΑΕΩΝ ΜΕΓΑC ΜΟΝΝΗCΗC, or ΒΑCΙΑΕΥC ΒΑCΙΑΕΩΝ ΜΕΓΑC ΜΟΝΝΗCΗC; of which I should chuse the former, notwithstanding it is such Parthian Greek, as the round of the medal seems not to have been capable of containing the latter. It will be, at first sight, observed, that this legend is different from those of the Parthian coins hitherto described; which are exhibited in the (2) genitive, not the nominative, case. The last two letters MO leave no room to doubt, tho' the O is not so extremely well preserved, that the piece was struck when Monneses sat upon the

(1) Nicol. Haym Roman. *Del. Tesor. Britan.* Vol. ii. p. 36, 37. In *Londra*, 1720.

(2) J. Foy Vaill. in *Arfacid. Imper.* Parisiis, 1728. *Numism. Antiqu. Collect. a Thom. Pembroch. et Montis Gomer.* Com. P. 2. T. 76. Nicol. Haym Roman. ubi sup. p. 30—38.

Parthian throne. It is true, indeed, the first element of the prince's name is not so apparently a *Mu*; tho' it resembles that letter, even as preserved on this very medal, much more than any other of the Greek alphabet. It is however totally unlike the *Alpha* near it, as well as all the other forms of that element to be met with in Montfaucon (3), and bears not the least resemblance to *Beta* (4), particularly the *Beta* which ought to have been impressed on this piece; as most evidently appears from a particle of that letter, visible in its proper place. From whence we may infer, that the character I am considering must be *Mu*; as *Alpha*, *Beta*, and *Mu*, were the only initial letters, according to Dr. Vaillant (5), of the names of the Parthian kings reigning after the introduction of the *Omega* of the minuscular form, as it here occurs, upon the Parthian coins. But Monneses was the only one of those princes whose name began with *My*, and consequently the two last Greek elements on this medal are part of his name. The metal and size of this piece, as none belonging to the other Parthian monarchs seem to have been yet met with of the same form, which exactly agree with those of Monneses's (6) coin published by Dr. Vaillant, may likewise be urged, as an additional argument of no small weight, in support of my opinion.

(3) Montfauc. *Palæograph. Græc.* p. 123, 124, 125. Parisiis, 1708.

(4) Idem *ibid.*

(5) J. Foy Vaill. *ubi sup.*

(6) J. Foy Vaill. *ubi sup.* p. 335, 341.

2. That the Greek and unknown legends on this medal are either of the same or a similar import, will be acknowledged by all versed in this kind of literature extremely probable. The Greek and Phœnician legends on the same (7) coins of Tyre and Sidon, as I have, upon examination, found, and shall hereafter more fully evince, clearly correspond. The Latin and Punic legends on Juba's medals, as has been by me formerly proved (8), very well agree. The sense (9) I have assigned the legend in unknown characters, exhibited by the reverse of my former Parthian coin, with, I flatter myself, some appearance of truth, sufficiently answers to the Greek one preserved by other medals of the same prince. We may therefore be permitted to suppose, that both the legends handed down to us by the coin before me related to Moneses, and conveyed the same, or at least extremely similar, ideas to the Parthians and the Greeks. Nothing can be more consonant to reason, tho' we must not directly assume this as a postulate, than such a supposition.

3. This notion will likewise receive a farther accession of strength from the characters of which the unknown legend is composed. The first of them so nearly approaches one of the forms of the Pal-

(7) Montfaucon. ubi sup. p. 118, 119. Hadrian. Reland. *Palaestina Illustrata*. Tom. ii. p. 1014, 1015, 1055. Trajecti Batavorum, 1714. I have a Latin dissertation in the press here, almost printed off, containing an explication of a considerable number of coins of Tyre and Sidon, with Phœnician legends upon them.

(8) *De Num. quibusd. Sam. et Phœn. &c. Dissert.* p. 53—56. Oxon. 1750.

(9) *Philosoph. Transact.* Vol. xlix. p. 593—607.

myrene *Pe*, as it appears in (10) Mr. Dawkins's alphabet, that we may without scruple ascribe to it the power of that letter. The second is so like the Palmyrene and the Chaldee *Daleth* (11), that it ought indubitably to pass for that element. The third differs something, tho' not greatly (12), from one of the forms of the Palmyrene *He*. The fifth, which likewise occupies the eighth place, is by no means remote from the figures of the (13) Palmyrene and Chaldee *Nun*. The sixth occurred in the third place before. With regard to the seventh, it seems to me pretty strongly to resemble some forms of the Palmyrene *Mem* (14), and even exactly to answer to that of the same letter in (15) one of the Palmyrene inscriptions preserved amongst those celebrated remains of antiquity commonly, tho' perhaps improperly, stiled *The Ruins of PERSEPOLIS*. The ninth is the *He* touched upon before. The tenth, which also succeeded the third, if the powers of the other elements have been rightly determined, must be *Schin*. Nor does this character, if we view it in a certain position, appear very remote from a rude form of that letter. This legend then, according to what is here advanced, as it now remains, consists of the words
 ----- PADESHANE MONESH, PADESHAN

(10) *Philosoph. Transact.* Vol. xlviii. p. 693.

(11) *Philosoph. Transact.* Vol. xlviii. p. 693, 740.

(12) *Philosoph. Transact.* ubi sup.

(13) *Philosoph. Transact.* ibid.

(14) *Philosoph. Transact.* ubi sup. p. 693, 740.

(15) Chard. *Voyages en Perse*, &c. Tom. iii. p. 119. A Amsterdam, 1711. *Philosoph. Transact.* Vol. xlix. p. *597, *598.

EMONESH, or (16) PADESHAN AMONESH, that is, MONESH, or AMONESH, ---- OF KINGS; the word PADISHAH, or rather PADESHAH, as it seems to have been written and pronounced by the (17) antient Persians, in the Pehlawian, Pehelawian, or Bastanian, that is, the old Persic, tongue, denoting (18) KING. That NI, or NE, was sometimes a masculine plural termination in the antient Persic, seems to appear from the word, or rather words,

و.ز.ی.ش.ن.ی. ل.ن.ی. BIR. ZEIVESHNI, LONGÆVI, which occurs in Dr. Hyde (19). And that the vowels A and E were sometimes prefixed to the Persian proper names, in the remoter periods of time,

(16) That the plural termination of PADESHAH, or SHAH, which, according to Khojah Afdhalo'ddin, denoted originally the same thing, was AN, or perhaps ANE, in the days of Ammianus Marcellinus, there is good reason to believe; the word SAANSAA, KING OF KINGS, having been then used by the Persians, and handed down to us by that author. The term ΣΑΑ, SAA, equivalent to the Persic SHAH, KING, likewise occurs in Agathias, a writer of the sixth century. Should my explication of the Parthian legend of the coin before me meet with the approbation of the learned, it will perhaps be granted me, that the plural of PADESHAH, or PADESHA, amongst the Parthians was PADESHAN, if not PADESHANE, in the second century after CHRIST. Hyd. *Hist. Rel. Vet. Pers.* p. 416. Khojah Afdhalo'ddin, D'Herbel. *Biblioth. Orient.* p. 767. Hadr. Reland. *Dissert.* viii. de *Vet. Ling. Pers.* p. 221, 222. Ammian. Marcellin. Lib. xix. cap. 2. Agath. Lib. iv. p. 135, 136. Parisiis, 1660. Ezech. Spanhem. *De Præstant. et Us. Numism. Antiquor.* Tom. i. p. 463---466. Lond. 1706.

(17) Hyd. *Hist. Rel. Vet. Pers.* p. 79. Oxon. 1700.

(18) D'Herbel. *Biblioth. Orient.* p. 699, 767. Hyd. ubi sup.
Hadr. Reland. *Differt. viii. de Vet. Ling. Perf.* p. 147. Trajecti ad
Rhenum, 1707.

(19) Hyd. ubi sup. p. 326.

is abundantly manifest, from the words SPYTTAMAN, ESFINTAMAN, or ESPINTAMAN, the (20) name of either the father or one of the ancestors of Zerâtusht; XERXES, (21) AXERXES, or AXERSES, and others of the same kind, that might, with equal facility, be produced. Whether therefore we read this legend --- PADESHANE MONESH, PADESHAN EMONESH, or PADESHAN AMONESH, we cannot greatly err. As the coin was not so perfectly struck, part of the Greek legend is thereby apparently lost; from whence, and the evident want of a word there, we may conclude, that the Parthian legend, for Parthian by the explication here laid down it appears to be, on the correspondent part of the reverse, must have met with the same fate. What that Parthian term was, I cannot take upon me to say; but the whole legend was probably something like THE GREAT KING MONNESES, MONNESES THE KING OF KINGS, or MONNESES (22) THE GREAT KING OF KINGS; all which titles are intirely consonant to those assumed by the Parthian kings (23), and transmitted down to us on their other coins. The Persian, or Parthian, termination of the proper name MONNESES, and others that occur, was ESH. This may be inferred from the legend now before me, in conjunction with Scripture (24), and has

(20) Hyd. ubi sup. p. 18, 312.

(21) Matth. Hiller. *Onomast. Sacr.* p. 619. Hadr. Reland. ubi sup. p. 259—262.

(22) Nicol. Haym Roman. ubi sup. p. 33.

(23) J. Foy Vaill. & Nicol. Haym Roman. ubi sup. & alib.

(24) Esth. c. x. v. 1.

been remarked by some good authors (25). That the Greeks also expressed the *Schin* by their *Sigma*, and superadded another termination to such words, is sufficiently manifest from the Persian, or Parthian, ARSHAK (26), which was converted into ARSACES by the Greek writers. I must farther observe, that, according to Herodotus (27), the antient letters of the Persians were those used by the Assyrians; which I take to have been the same with the elements that prevailed amongst the Syrians, and formed the alphabet of the Palmyrenes. Nay, we may collect from (28) Epiphanius, that a considerable part of the Persians used the Palmyrene characters as late as the decline of the fourth century after CHRIST. All which being maturely weighed, I cannot forbear thinking the interpretation of the Parthian legend here laid down in a good degree probable. I am far however from insisting upon the truth of it. I shall only beg to be indulged the liberty of proposing it to the consideration of our most illustrious Society, and leave the fate of it intirely to the decision of so very eminent a part of the learned world.

4. But however my explication of the Parthian legend may be received, I believe it will scarce be denied, that the coin was struck in the reign of Monneses, one of the Parthian kings; this point

(25) Scalig. *Prolegom. in Lib. de Emend. Temp.* p. 41. Col. Allobrog. 1629. & *Can. Isag.* f. 317. Matth. Hiller. ubi sup. p. 619, 620.

(26) Moses Chorenens. *Hist. Armen.* Lib. ii. iii. Theophil. Sigefr. Bayer. *Hist. Osroen.* &c. p. 97. Petropoli, 1734.

(27) Herodot. Lib. iv. c. 87.

(28) Epiphani. *Adv. Hær.* Lib. ii. Tom. ii. p. 629. Paris. 1622.

having been so clearly evinced, by the reasons above alledged. It may not be improper here to remark, that the republic of letters has been obliged with the publication of two of this prince's medals; the first of which was (29) coined in the year of the Parthian æra 422, and the other in 425 (30). Neither of them however exhibits the *Victory* impressed on the reverse of mine. That symbol, adopted by the Parthians in imitation of the Roman manner, must undoubtedly have alluded to some victory, or at least some remarkable advantage, gained by the Parthian forces over the Romans, a little before the piece was struck. What that advantage was, when and where obtained, and whether history conspires with the medal, in order to settle this point, I am next to inquire.

5. Monneſes, if Dr. Vaillant (31) may be credited, ascended the Parthian throne in the year of the Arſacidæ 422, nearly coincident with the 166th of CHRIST, when M. Aurelius and L. Verus presided over the Roman world. But so far were the Parthians at that time from gaining any victories over the Romans, that they were every where worsted by them, and put to the rout. A Roman army, under the command of Avidius Cassius, had penetrated into Mesopotamia and Assyria, ravaged those provinces, laid Seleucia in ashes, taken Ctesiphon, and levelled the royal palace there with the ground,

(29) J. Foy Vaill. ubi sup. p. 339.

(30) Maffei, in *Gall. Antiqu. Quæd. Select. Epist.* 22. p. 106. Pariliis, 1733.

(31) J. Foy Vaill. ubi sup. p. 334, 335, 336, 339.

according to Dio (32), the preceding year. Nay, it appears both from (33) Capitolinus and the Roman (34) coins, that M. Aurelius and L. Verus triumphed over the Parthians, the very year after Monnefes, as (35) Dr. Vaillant will have it, came to the crown. All which that celebrated antiquary acknowledges to be true; and adds, that Monnefes concluded an infamous peace with the Romans, ceding to them the whole province of Mesopotamia, for which he was soon after deposed by his subjects. Here then we can discover not the faintest traces of a reason for the appearance of a *Victory* upon the medals of this prince. Nor does any thing like a reason for such an appearance, in antient history, occur, before the 950th year of Rome, corresponding with the 197th of CHRIST; when the Parthians, animated by the civil dissensions, which reigned amongst the Romans, Albinus and Severus then fiercely contending for the empire, entered Mesopotamia with a powerful army (36), and reduced to their obedience most of the cities of that

(32) Dio, Lib. lxxi. p. 802. *Imperator. Romanor. Numism. &c. Stud. & Cur.* Francisc. Mediobarb. Birag. p. 218. J. Foy Vaill. *Can. Chronologic. Reg. Parthor.* p. 41.

(33) Jul. Capitolin. in *M. Antonin. Philos. et in Ver.*

(34) *Imperator. Romanor. Numism. &c. Stud. & Cur.* Francisc. Mediobarb. Birag. S. R. I. Com. &c. p. 220. Mediolani, 1683.

(35) J. Foy Vaill. ubi sup. & in *Arfacid. Imper.* p. 338.

(36) Dio, Lib. lxxv. p. 853. J. Foy Vaill. in *Arfacid. Imper.* p. 356. & in *Can. Chronologic.* p. 42. Ludovic. Du Four de Longuerue, Ab. S. Joan. de Jardo ad Melod. et Sept. Font. in *Therasc. Annal. Arfacidar.* p. 51. Argentorati, 1732. Erasim. Froelich, S. J. S. in *Dub. de Minnifar. Aliorumque Armen. Reg. Num. et Arfacidar. Epoch. nuper vulgat.* p. 66. Viennæ Austriæ, 1754.

province. Now if, with F. Corfini (37), we admit the commencement of the Parthian æra to have happened in the 525th year of Rome; the 425th or 426th year of the former and the 950th of the latter of those æra's, wherein the Parthians undertook the Mesopotamian expedition, will nearly coincide. But it may be proved from a medal of Monneses, described by the (38) Marquis Scipio Maffei, in opposition to (39) Dr. Vaillant, that this prince was not dethroned in the year of the Arfacidæ 423, but had two years afterwards the management and direction of the Parthian affairs: wherefore, notwithstanding what has been advanced to the contrary by that antiquary, he might still have sat one or two years longer upon the Parthian throne. This may likewise be inferred from Dio, who mentions Vologeses as presiding over the Parthians, not in 198, but in 199, about two years after their irruption into Mesopotamia. All which being with proper attention considered, it will appear extremely probable, that the medal I have been endeavouring to explain was coined either in the 425th or 426th year of the Parthian æra, that is, the 197th of CHRIST; that the authority of (40) Arrian, (41) Justin, and (42) Athenæus, on which

(37) Edv. Corfin. Cl. Reg. Scholar. Piar. in Acad. Pisan. Humanior. Litterar. Profess. *De Minnisar. Aliorumq; Armen. Reg. Num. et Arfacid. Epoch. Dissertat.* p. 13—29. Liburni, -1754.

(38) Maff. in *Gall. Antiqu. Epist.* 22. p. 106.

(39) J. Foy Vaill. ubi sup. p. 338.

(40) Arrian. in *Parthic.* apud Photium, *Cod.* 58. & apud Syncell. in *Chronograph.* p. 226.

(41) Just. Lib. xli. c. 4.

(42) Athen. *Deipnosoph.* Lib. iv. c. 13.

F. Corfini principally founds his notion, may be intirely depended upon; and, in fine, that the arguments he makes use of on this occasion, to (43) evince the truth of his scheme, however they may be opposed by F. Frœlich, and the medal before me mutually strengthen and support one another.

6. I must not forget to remark, that F. Corfini thinks, without a proper foundation, that the piece of Monneses published by Dr. Vaillant may be considered either as a Parthian or (44) an Armenian coin. This, I say, he takes, without a proper foundation, to be the case. For the medals of the Armenian kings, such as that he has obliged the learned world with a draught of, and a most excellent dissertation upon, discover a taste far different from that which is exhibited by Dr. Vaillant's coin. The air of the face, the curls into which the hair is formed, and in fine every thing else visible upon the former, except the symbols on the reverses, bear little resemblance to what is presented to our view by the latter. As for the titles, impressed on these medals, they are far from being of the same kind; the Armenian princes in this particular approaching nearer the successors of Seleucus (45), and contenting themselves with more plain and simple titles than that lofty one affected by Monneses, according to Dr. Vaillant, in common with the other Parthian kings. To which I may add, that my coin sets this point beyond dispute, by the Parthian characters it has so apparently preserved; all the Armenian medals I have hitherto met with,

(43) Corfin. ubi sup.

(44) Corfin. ubi sup. p. 2.

(45) Erasim. Froel. ubi sup. p. 72.

about three or four in number, as well as that of Baron Stofsch (46), which F. Corfini has so learnedly explained, having only Greek legends upon them. What therefore he has advanced, on this head, must be considered as not altogether so consonant to truth; especially, as he seems to have offered nothing of any great weight in support of his opinion.

7. Before I dismiss the present subject, I must beg leave to take notice of the Parthian character on the field of my medal, which is not very unlike the Chaldee (47) or Palmyrene *Aleph*, tho' of something a more complex form. Should this be allowed, that character may be considered as the initial letter of ARSACIA, the name of a city subject to the Parthians, placed by (48) Ptolemy in Media; where, according to (49) Dr. Vaillant, many of the Parthian medals were coined. That city is however believed by some to have been seated on the spot occupied at this time by Casbin, or rather Kazwîn (50), one of the principal towns of the Belâd Al Jebâl, or mountainous part of the Persian Itâk; for a particular account of which place, the curious may have recourse to Golius, and the eastern geographers. As I have already far exceeded the limits proposed to myself, when I began this letter, I shall not now touch upon any other observations relative to the

(46) Corfin., in *Des. Nub. Vir.* Phil. Veneti p. 5.

(47) *Philosoph. Transact.* Vol. xlviii. p. 693, 740.

(48) Ptol. *Tab. Urb. Insign.* p. 39. Ed. Hudf. Oxon. 1712.

(49) J. Foy Vail. in *Arsacid. Imper.* pass.

(50) Hamdalla Ism. Abu'Kêl. Mohammed Al Firauzabad. Nâsîr Al Tûsî, Ulugh Beik, &c. Golii not. ad Alfragan. p. 200.

medal before me, which is the only one I have hitherto met with carrying a Greek and a Parthian legend upon it; but only assure you that I am, with the most perfect regard,

S I R,

Your most obliged,

Christ-Church,
Oxon. Nov. 29th,
1756.

and most obedient Servant,

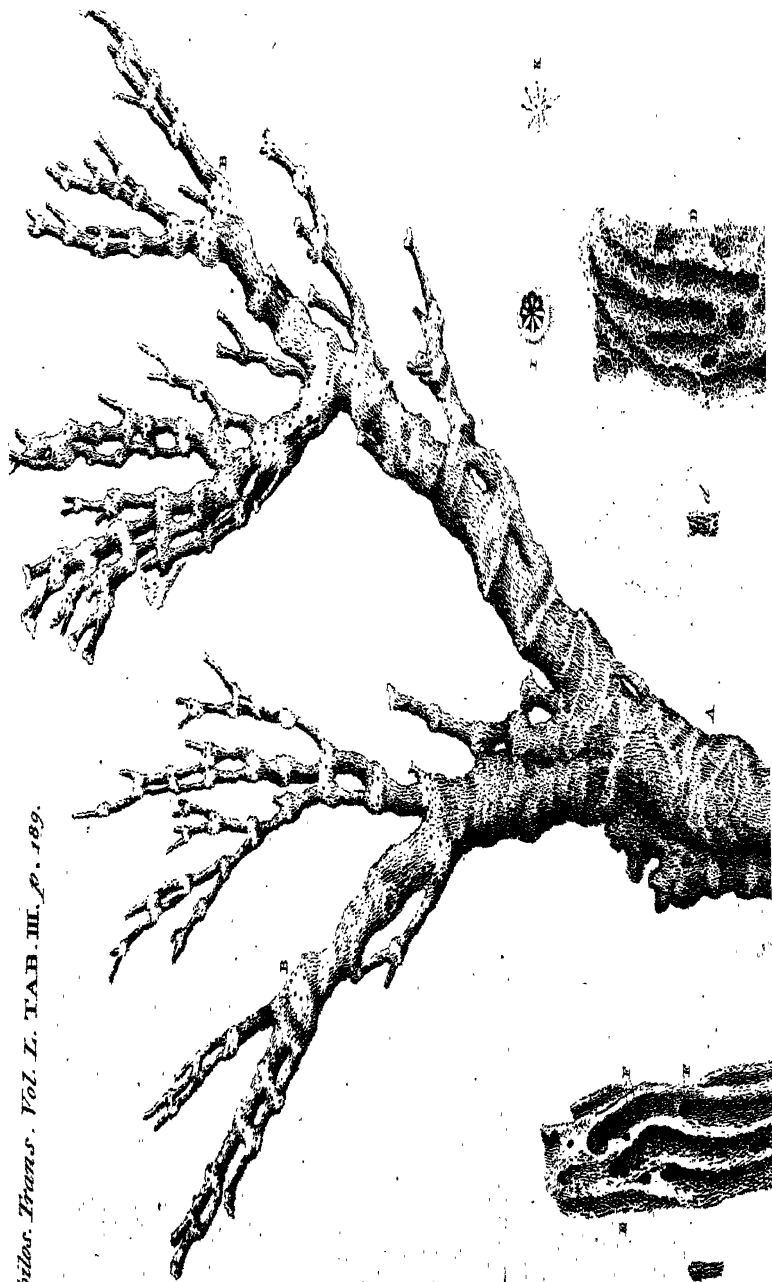
John Swinton.

XXIII. *An Account of a Red Coral from the East-Indies, of a very singular Kind: In a Letter from Mr. John Ellis, F. R. S. to Mr. Peter Collinson, F. R. S.*

Dear Sir,

Read Mar. 24, 1757. **I** Promised you, in my letter of the 7th of February 1754, published in our Transactions, Vol. xlviii. p. 507. that I would, when I had an opportunity, endeavour to illustrate the tubular structure of the common red coral of the Mediterranean sea, and of some of the keratophyta; which two kinds, tho' evidently of as different natures as stone and horn, yet are, from late observation, found to be fashioned, or raised up into those beautiful forms, by animals of the same class.

This I shall attempt to do, by comparing them with bodies of a similar kind, but of a less compact texture: for which reason I formerly referred you, in the above-mentioned letter, to a figure, which I have given of the herring-bone-coralline, with its animals

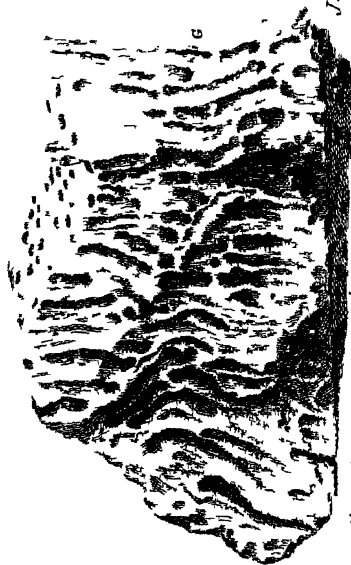




A. A piece of poplar knothole and Canal in the pith, colored in the Portulaca y hulk appears to be formed by animals of the Oblique kind, contrary to the recent opinion of these holes being ingested by the vermicular tubes of a hulk it is composed, being no more than the scars or coverings of the heads of these insects.

BB The radiated holes on the yellow nearly surface through which these insects entered their arms or claws, as in the very much red, coral.

CCG Small branches almost covered by aphids, each



stem of pines, that has grown all round and increased the bulk of the stem, continuing themselves along it to extend the ramifications by this means they are like the former, and branches into their own stems, differing entirely from the larva of vegetation.

D. A piece of knothole, points magnified to show their reticular but here like that of Oblique range of tubes.

d. The same in its natural size.

E. A piece of blood red but which is hard and has a slightly rough and surface with some appearance of the cotton, but not from its pith, it is a very knot.

J. Myndale

e. The same in its natural size.

FF Small holes in its bulk, spread by a branch of its tubes.

G. A single of the stem not so highly magnified, when they are mostly full of sapling from down to its top.

H. The same in its natural size.

I. The transverse sections of the same show a ring the holes, in the ends of the tubes.

K. One of the radiated tubes magnified as shown by G, pointed, below, in which it is one and a half the height shown in its head in both is stopped at K, and opens out and a half it is not.



9

animals alive in it, *Phil. Transf.* Vol. xlviii. TAB. XVII. *Fig. E, F, G,* to shew you the nature of the tubular structure of the keratophyta.

I now lay before you a piece of red coral (*See TAB. III. Fig. A.*) from the East Indies of a very singular kind, which I received from your friend Abraham Hume, Esq;. The stem and branches of this appear evidently to the naked eye to consist of a combination of vermicular tubes closely connected together: and, if we trace these little tubes to their starry openings on the surface, *Fig. B.* we shall plainly discover them to be the red testaceous coverings of certain marine polypes, which have raised themselves thus upright, and disposed themselves into this remarkable vegetable form.

In order to form some idea, how these masses are increased and extended to the sizes we often meet with them, and where the same regularity of shape is preserved in the large, that we find in the small; we think it more than probable to suppose, that the species of polypes, that compose this coral, breed as we find all other polypes do: and this appears more evident to me, from what I have already discovered in many kinds of corallines (*See Plate 38. of my Essay on Corallines*), where the young polypes in some species are produced in the egg state, while others fall in great numbers from their matrices, completely formed, down to the roots of their parent corallines, either to begin a new race of the same species near them, or to increase the trunk, and extend the ramifications, of the plant-like figure which they just descended from.

From observing this method in nature, we shall the easier account for the progress of those generations

tions of young testaceous polypes of this coral ; which appear to us succeeding each other, and raising themselves up from the root or base, passing along the stem and branches, and covering the whole anew with their shelly cases : and in this their passage upwards we may observe, in the specimen before us, how they have involved and incruited the small lateral branches of the former generation, so as almost to hide their appearance. From hence we may trace them extending themselves to the extremities of the upper branches, and there forming a new series of slender twigs, proportionable to those which they had just covered, still keeping order and exact symmetry in the whole structure.

The distinguishing character of this red coral, after we have considered its fistulous texture, is the knotty joints, of which it is composed : these appear more distinct, and are placed at a greater distance, in the smaller branches than the large ; and, as we descend to the trunk, the traces of these inequalities but just appear.

From these protuberances, or knots, the lateral branches take their rise ; and as these and the leading branches grow up together, they frequently inosculate at these joints, forming a kind of network, like what we observe in many of those species of *keratophyta*, which are called sea-fans.

The surface of this coral, when recent, is covered with a mealy friable matter, of a yellow colour, not unlike that of the true red coral, but much fuller of little raised starry cells. The figure of these cells is owing to the radiated position of the claws of the polypes.

Upon removing this friable matter, we observe,
that

consider this covering, for the future, as proper and necessary for the well-being of these little animals, as they do at present hair and wool for beasts, feathers and down for birds, and scales and slime for fishes.

This red coral is mentioned by Rumphius, in his *Herbarium Amboinense*, Vol. vi. Tab. 85. p. 264. but, as the figure he has given, is not sufficient to demonstrate its tubular texture and animal structure, I have had it more accurately drawn; and those parts in particular magnified, which may tend to illustrate the foregoing description. He mentions, that it is in great esteem with them, on account of its beautiful figure; but would be much more so, if it was not for the great difficulty they find in preserving the smallest twigs from falling off; which is the reason, I suppose, that this specimen is not more complete.

Lastly, he tells us, that it is used by the inhabitants of the Spice-islands as a principal ingredient in their medicines to expel poison: as also, that they have it in great esteem on account of its excellent diuretic quality.

Upon examining this coral in the microscope, I observed, that the outside tubes of the stem were chiefly stony, but that the inward parts were composed of as many divisions of spongy tubes, as there were of stony ones.

This I find arises from the smaller ramifications, which being spongy at the knobs, and stony in the spaces between them, are inclosed and united together into one common mass during the growth of this coral; so that both the soft and hard parts together make up the inside of its trunk or stem. When we examine minutely the two parts, that
compose

compose the branches, we find, that the knobs consist of little sponge-like tubes interwoven together, as they appear magnified at *Fig. D*; and the shank or part between the knobs is composed of stony tubes, that are more erect (*See the piece magnified at E*): these tubes appear to be branched from the lateral holes at FF. The *Fig. E* likewise shews the appearance of the tubes on the surface of the main stem.

The radiated openings in the little wart-like figures on the surface of the branches are guarded by eight pointed valves, as magnified at *Fig. I.*: these inclose the heads of the polype, one of which is figured at K.

The stem of this specimen is so intirely divested of its yellow mealy covering, that we may easily trace the manner in which the animals, that compose it, have carried up their stony tubular cells, which lie side by side along the surface. These tubes have still some marks of sponginess at particular distances, which, as they come to join together, form those irregular cross-lines, that surround the stem in several places. *See Fig. A.*

In other specimens I have observed the principal stems covered over with calcarious tubes, such as I have described in the *Essay on Corallines, &c.* in that species of keratophyton called Venus's Fan. *Plate 26.*

The sponginess of the knobby joints occasions that excessive brittleness in the lesser branches; which accounts for the difficulty, which Rumphius mentions, of getting good specimens of this beautiful coral.

I have lately seen a white pipy and stony coral
 VOL. 50. C c with

with spongy knobs, which is only a different species of this genus, in the very curious cabinets of our friends Dr. John Fothergill, M. D. and Mr. Isaac Romilly, F. R. S. specimens of which they have both lately received from the East Indies. The examining of these has given me an opportunity to be more particular in the description of this coral; which seems to point out to us the great affinity there is between corals, keratophyta, and sponges.

I am,

Dear Sir,

Your most affectionate humble Servant,

Laurence-Lane,
Mar. 24. 1757.

John Ellis.

XXIV. *An Account of the Effects of a Storm at Wigton in Cumberland. Communicated by Mr. Philip Miller, F. R. S.*

To the Rev. Tho. Birch, D. D. Secr. R. S.

S I R,

Read Mar. 31, 1757. **I** Received the inclosed letter by the post, giving an account of the storm, which happened lately in the north. If the Royal Society have not already been informed of the effects of it, and you think the contents of it worthy their notice; I beg you will be so good as to communicate it to them. The facts therein mentioned have been confirmed to me by a person of skill and integrity.

tegrity. Mr. Thomlinson's conjecture of the cause of the leaves of trees appearing scorched after the storm, I believe to be true; having two or three times myself observed the same in Suffex, at a considerable distance from the sea; when all the hedges, trees, and woods, on the side toward the sea, have had their leaves scorched, as if fire had passed over them; and their opposite sides from the sea have continued in full verdure; which frequently happens in storms from the south-west: and, upon tasting their leaves, I have found them as salt, as if they had been steeped in brine. I am,

S I R,

Your most obedient humble Servant,

Chelsea, Nov. 23.
1756.

Philip Miller.

To Mr. Philip Miller.

S I R,

Wigton, Nov. 15. 1756.

ON the 6th of last month, at night, happened a most violent hurricane, such an one, perhaps, as has not been known in these parts in any one's memory. It lasted four hours at least, from about eleven till three. The damage it has done is very deplorable. The corn has suffered prodigiously. Stacks of hay and corn have been intirely swept away: houses unroofed, and in several places driven down by its fury: trees without number torn up by the roots; others snapt off by the middles, and their fragments scattered over the adjoining fields. Some were twisted almost round, or split down to the very
C c 2 ground;

ground; and, in short, left in such a shattered, mangled condition, as scarce any description can give you an adequate idea of.

The change in the face of the country was very surprising in one single night: for, to complete the dismally-desolate scene, the several tribes of vegetables (in all their verdure the day before), as if blasted with æthereal fire, hung down their drooping heads. Every herb, every plant, every flower, had its leaves withered, shrivelled up, and turned black. The leaves upon the trees, especially on the weather side, fared in the same manner. The evergreens alone seem to have escaped. The grass also, in a few days time, recovered itself in a great measure.

I agreed at first with the generality of people in their opinion, that lightning had done all this mischief: but upon recollecting, that there had not been much seen any where, in many places none at all, but that the effect was general (1), as far as ever the wind had reached; I began to think, that some other cause might probably be assigned. Accordingly, I set myself immediately to examining the dew or rain, which had fallen on the grass, windows, &c. in hopes of being enabled, by its taste, to form some better judgment of the sulphureous or nitrous particles (or of whatever other quality they were), with which the air was so strongly impregnated that night, as to produce such strange effects. Nor was I deceived in my expectations: for, upon tasting it,

(1) Upon inquiry, I find, that no such thing was taken notice of in Northumberland; so it probably has not extended any further to the eastward, than the skirts of our own county.

I found it as brackish as any sea-water. The several vegetables also, which I tasted, were all salt, more or less, and continued so for five or six days after; the saline particles not being then washed off; from the corn and windows in particular; the latter of which, when the moisture on the outside was exhaled next day, sparkled and appeared exceeding brilliant in the sunshine. This saltiness, I conceive, has done the principal damage: for common salt dissolved in water, I find, upon experiment on some fresh vegetables (when sprinkled two or three times upon them) has the very same effect, except that it does not turn them quite so black: but particles of a sulphureous, or (2) other quality, may have been mixed with it. That this salt water had been brought from the sea (3), every body, I think, will allow; but the manner how (4), is not so easy to conceive.

This freedom, Sir, perhaps may want an apology: but, as a gentleman (5) of the Society you have the honour to be a member of, did not think something of the like nature either unworthy of his own notice, or that of the world; and as the hurricane principally affected these parts of nature, in the knowledge of which you have so eminently distinguished your-

(2) In an adjoining bleach-yard, a piece of cloth, which had been left out all night, was turned yellow; and was not without some difficulty washed out again. Some also, which was spread out the next day, contracted the same colour.

(3) The wind was westerly, and consequently would sweep the Irish sea.

(4) No rain, or however very little, during the hurricane.

(5) Mr. Derham, in his *Physico Theology*.

self;

self; I flattered myself you would excuse the trouble I should give you in a perusal of an account of this very strange, tho' hitherto unnoticed, phenomenon.

I am, Sir, with the greatest respect and esteem,

Your most humble Servant,

Thomas Thomlinson.

XXV. *An Account of the Effects of
upon the Steeple and Church of Lestwithiel,
Cornwall; in a Letter to the Right Ho-
nourable the Earl of Macclesfield, Presi-
dent of the R.S. By Mr. John Smeaton,
F. R. S.*

Read April 28, 1757. **J**ANUARY 25. 1757. about five o' clock in the evening, returning home from the Edystone works near Plymouth, I observed four flashes of lightning, within the space of six or seven minutes, towards the west; but heard no noise of thunder *. A few days after, I was informed, that the same evening the lightning had shattered the church of Lestwithiel in a very surprising manner.

The 1st of March I was at Lestwithiel: they had then begun to repair the damages; but had not made such a such progress, but that the principal effects were equally observable as at first. I observed, and was informed, as follows:

* Distance about thirty miles,

At the time before-mentioned, the inhabitants were alarmed by a violent flash of lightning, accompanied with thunder so sudden, loud, and dreadful, that every one thought the house he was in was falling upon him; almost every one being within doors, on account of a violent shower of rain, which preceded the lightning: so that no body saw or heard any thing of the mischief done to the church, till it was observed accidentally after the shower.

The steeple is carried up, plain and square, to about 49 feet, with a kind of slate-stone, rough-cast on the outside; upon which is formed a very elegant octagon Gothic lanthorn about 9 feet high, and thereon a stone spire about 52 feet height, with a spindle and vane rising about 3 feet above the stone: so that the whole together was about 113 feet. Each face of the lanthorn finishes above with a sort of a Gothic pediment, with a little pinnacle upon each, separated from the body of the spire.

I will not affirm, that the lightning entered in at the spindle or vane at top; but will suppose it, for the sake of methodizing the facts. The vane was of plate copper, which being turned round, and rivetted, made a socket to turn upon. The spindle did not reach thro' the socket, but the weight of the vane rested upon the top of the spindle, the top of the socket being closed. About the vane were many acute angles, and some almost sharp; but I did not observe any pointing directly upward. The vane was much bruised, which might be occasioned by the fall; but the socket was rent open, as if it had been burst by gunpowder; and in such a manner, as I cannot conceive could be occasioned by the fall.

Under

Under the spindle, that carried the vane, was a bar of much the same size and length †, that passed thro' the center of several of the uppermost stones successively, in order to unite them the more firmly together, and was run in with lead: all which surrounding stones were broke off, except one, which, together with the bar, fell down within the tower.

The shell of the spire, as far down as 35 feet from the top, was no more than 7 inches thick, and the courses about the same height: so that scarce any one stone in the spire could weigh more than 30 or 40 pounds; but they were joined together at the ends with mortise and tenon, in a curious manner. Above 20 feet of the upper part was intirely thrown down, and dispersed in all directions; and, as I was informed, some pieces were found at the distance of 200 yards. A great many stones fell upon the roof of the church; and several made their way thro' both roof and cieling down into the church, breaking the pews, and whatever they fell upon. Six feet still lower the spire was separated; the westermost half being thrown down; the eastern half was left standing, but disjointed, and in so critical a posture, that it seemed ready to fall every moment: so that this was ordered to be taken down immediately; and likewise to 6 feet below, the work being found remarkably shattered. In this condition it was when I saw it. The whole of the spire I found much cracked and damaged, but the remainder of the 7 inch shell so greatly, that there seemed scarcely a whole joint.

† *Viz.* About four feet long, and one inch square.

The pediments over every face of the lanthorn were damaged more or less; but the whole ashlering of that to the N.W. was torn off from the inner wall, to which it was connected. At first sight this might seem to be done by the falling of the stones from above; but I was convinced to the contrary, by observing, that several of the pediments were damaged, and even stones struck out, where the little pinacles above them were left standing.

About the top of the lanthorn is a bell for the clock to strike on: it is hung upon a cross-bar, with gudgeons at each end; the whole being suspended to a beam laid across the tower. The cross-bar was so bent, that the clock-hammer would not touch the bell by above 2 inches. This could not be done by the falling of stones, because the beam would defend the bell from receiving any stroke in the direction to which the cross-bar was bent. As to the wire, that drew the hammer, as I was informed, not one bit of it could be found.

The bells (four in number) for ringing hung in the square part of the tower, below the lanthorn, two above and two below: the wheels of every one were broke to pieces, and one of the iron straps, by which they are fastened to the yoke, unhooked; and, as appeared to me, could not be replaced without great force, or unloosing. Whether these accidents were occasioned by the lightning, or the falling stones, I leave undetermined.

In the floor under the bells was placed the clock, cased up with slight boards. The verge, that carries the pallets, was bent downwards, as if a ten pound weight had fallen ten feet high right upon it. The

been drove from above thro' the joints of the stone, and thereby forced off the plaister with its end. The lath and plaister partition, which stopped up the upper part of this door-way, was forced into the church, and the wainscotting making the back of the last seat was torn from the wall from end to end. Some part of the vapour seems to have made its way thro' the cavity under the seats; for most of the boards composing the rise of the steps from seat to seat were blown out forwards; and several panels of wainscot at each end of the seats, at *d* and *e*, were forced out, and broke. Hence the vapour seems to have divided itself into three branches; one moving directly forward to the east window G, being 13 feet wide, and about 20 feet high, consisting of five principal lights divided by stone mullions: two of the lights were in a manner wholly destroyed, and several large holes in those remaining; the glass and lead being carried outward, like as if an harlequin had leaped thro' the window. The north window E, fronting the broken panels at *d*, was very much shattered: but the south window F had scarce a whole pane left.

It is farther to be noted, that almost all the lights in the church, tho' not broke, were bagged outward; but those parts remaining intire in the window D most remarkably so.

N.B. It was said in the London papers, that the organ was intirely spoilt: it is certain there is not, nor ever was, any organ in this church.

XXVI. *An Account of the Case of the late Right Honourable Horace Lord Walpole; being a Sequel to his own Account published in the Philosophical Transactions, Vol. xlvii. p. 43. and 472.*

I.

Copy of a Letter from John Pringle, M. D. F. R. S. to Dr. Robert Whytt, Professor of Medicine in the University of Edinburgh, and F. R. S. relating to the Case of the Right Honourable Lord Walpole; with Dr. Whytt's Answer. Communicated by Dr. Pringle.

S I R,

London, 22 Feb. 1757.

Read April 21,
1757.

I Imagined, that upon hearing of Lord Walpole's death, you would be desirous to know the state of his Lordship's health from the time he published his own case; whether he continued the use of his medicines to the last; what distemper he died of; and, if his body was opened, what was the condition of his bladder and kidneys. I informed myself, as well as I could, of all these particulars; and I hope I shall be able to give you some satisfactory account of most of them.

Last year, in the month of March, about ten months before his Lordship's death, I happened to meet him at a friend's house, where he dined, and never saw any man of his age with a more healthful appearance. He was then in his 78th year. He eat
with

with an appetite, and of a variety of dishes ; drank some Madeira; and was very chearful the whole time. His Lordship then told me, that he had enjoyed perfect health since he sent his case to the Royal Society ; that he thought it probable there was still a stone in his bladder, but so diminished, or smoothed, as to give him no uneasiness ; that he did not think it safe to go about the streets of London in a coach, but that he went every where in a chair ; and that, in the country, he could travel 40 miles a day in his post-chaise, without fatigue, or feeling any of his old pains upon the motion. That he continued to drink, for a constancy, three pints of oyster-shell lime-water daily ; and to take, as often, from half an ounce to a whole ounce of soap, by way of lenitive. All these circumstances I am sure of, because I noted them down when I came home.

From this time to the beginning of winter, Lord Walpole (as Mr. Graham, his apothecary, informed me) continued in the same state of health ; but some time after coming to town, his Lordship was seized with a lingering feverish disorder, very much affecting his spirits, but intirely unconnected with the stone. Dr. Shaw, who attended his Lordship for about a fortnight before his death, told me, that there had never been any stoppage of water, or passing of bloody urine, or any pain about his bladder or kidneys, during his last illness ; but that he now and then felt some irritation in making water, a symptom too inconsiderable to require any other medicine than the continuation of his lime-water ; which, in a smaller quantity, he drank till within two or three days of his end.

Mr.

Mr. Ranby and Mr. Hawkins, surgeons, with Mr. Graham, were present at the opening of the body; and from the two last I received the account of the dissection.

The coats of the bladder appeared to be a little thicker than natural, but were otherwise sound. The *glandula prostatica* was of a large size, but not distended. They found three *calculi*, two lying loose in the bladder, and the other, a very small one, sticking in the passage, at that part, which is surrounded by the prostate gland. Mr. Graham favoured me with a sight of them all. The two first were very much alike, being of the shape and size of the kernel of a Spanish nut; only the sides were irregularly flattened, but without forming any sharp angle. The surface of each was every where smooth, except where there had been a separation of some small scales, not so thick as one's nail; and the largest exfoliation from one of these stones appeared to have been nearly about the breadth of the nail of my little finger. The polish otherwise, as well as the colour of both, might be compared to a boy's marble. One of these *calculi* weighed 21 grains, the other 22 grains: they were heavy for their bulk, and seemingly of a hard substance. The smallest stone having been put up with some others of the same size, taken out of the gall-bladder, Mr. Graham could not be positive which of them it was; and therefore I can only say, that what he thought most likely to be so, was about the size and shape of the seed of an apple, with the point broken off and the edge ragged. This, as I observed, was found in the passage, seemed to be coming away, and probably had occasioned that irritation

tation the patient had now and then felt during his last illness. It weighed only about a grain.

No parts could have a foundier appearance than both the ureters and kidneys. The first were not dilated; nor did the last contain any stone, mucus, or gravel: the pelvis in each was of a natural size.

The rest of the abdominal *viscera* were in the same healthful state, except the gall-bladder, which was full of stones. The largest was about the size of a small chefnut, but rounder. The surface was smooth, particularly at one part, where it seemed to have rubbed upon a lesser *calculus*, of the shape of one of the *vertebræ* of a small animal, without the processes. This last had a hollow on each side corresponding to the convexity of the large stone; and these cavities being finely polished, it seemed as if sometimes one side, sometimes the other, of the small stone had been turned to the great one, and had been shaped in that manner by the attrition. The largest *calculus* weighed one drachm two scruples and two grains; the small one but nine grains: they both sunk in water; and felt specifically heavier than any stones I have ever seen taken out of the gall-bladder. Besides these two, there were several very small *calculi* of irregular shapes, and of rough surfaces, which all together did not weigh above five grains. Mr. Graham, who had attended his Lordship for about 40 years, assured me, that he never had any symptom that indicated a stoppage of the bile, or the passage of a stone from the gall-bladder into the intestines.

Neither the head nor breast were opened.

These are all the materials, I can furnish you with, relating to this case. If you desire to be more particularly

cularly informed of any of these circumstances, let me know, and I will endeavour to procure you all the lights I can. In the mean while, I should be glad to have your remarks upon what I have now sent you; and since you have been so long in the train of thinking, with more than usual attention, on this subject, I presume it would be very agreeable to the gentlemen of the Royal Society to have a paper from you on this occasion; and the rather, as his Lordship began his course of soap and lime-water, upon hearing of your success by that method of cure. I am,

S I R, &c.

John Pringle.

II.

Some Observations on the Case of the late Right Honourable Lord Walpole, of Woolterton: In a Letter to Dr. John Pringle, F. R. S. By Robert Whytt, M. D. F. R. S.

S I R,

Edinburgh, March 16. 1757.

Read April 21, 1757. **P**HYSICIANS have not, perhaps, differed more widely in any thing, than in their opinions of the medicines lately proposed for the cure of the stone. While some imagined, that Mrs. Stephens's medicines, or soap and lime-water, were in most cases to accomplish a dissolution of the stone; others have been positive, that nothing of this kind was to be expected from them: nay, they have condemned these medicines, when used in large quantities, and long persisted in, as hurtful to the

stomach, guts, and urinary passages; and have ascribed the remarkable ease, which they almost always give to calculous patients, to their depositing a calcarious powder upon the surface of the stone, by which it is rendered less hurtful to the bladder. And this opinion seems to have been not a little strengthened, by the great quantity of white sediment observed in the urine of those patients, who have used soap and lime-water in considerable quantities. Now, as I am of opinion, that most of these objections and doubts, concerning the effects of soap and lime-water in the cure of the stone, may be cleared by a candid consideration of Lord Walpole's case, I shall trouble you with a few remarks, which have occurred to me, in comparing it with the appearances found in his Lordship's body after death, of which you were so obliging as to send me a particular account.

1. Whatever doubts may have been entertained concerning the cause of Lord Walpole's complaints, yet it now appears evidently beyond dispute, that they must have been owing, not to a scorbutic corrosive humour in his bladder, as was imagined by some (1), but to stones lodged in it. These stones may possibly have lain there since 1734; for from that time to spring 1747, his Lordship was free of any gravelish complaints, only passing some red sand at times. But at what time soever they may have first arrived in the bladder, in 1747 and 1748 they

(1) Philosoph. Transact. Vol. xlvii. p. 48. and Essay on the Virtues of Lime-water, &c. edit. 2d. p. 197.

seem to have acquired such a bulk, or were become so rough or pointed in their surface, as to occasion great pain, frequent provocations to urine, and sometimes bloody urine; especially after any considerable motion. These complaints, however, were soon relieved, by swallowing daily an ounce of Alicant soap, and three English pints of lime-water made with calcined oyster-shells: and from 1748 to 1757 his Lordship was kept almost intirely free from any return of them, except for some months of 1750 and 1751, during which he took only one-third part of the quantity of soap and lime-water above-mentioned (2).

2. It is highly probable, nay, I think, altogether certain, that the soap and lime-water not only relieved Lord Walpole of the painful symptoms occasioned by the stones in his bladder, but also prevented their increase.

If these stones came into the bladder in 1734, they must, in so many years as his Lordship lived after this, have acquired a very great bulk: nay, if we suppose them not to have been lodged in the bladder above a year before they began to occasion frequent inclination to make urine, with pain, and sometimes sudden stoppages of urine; yet, from 1746 to 1757, they ought to have grown to a much larger size than that of the kernel of a Spanish nut (3). 'Tis

(2) Philosoph. Transact. Vol. xlvii. p. 48 and 473. and Essay on Lime-water, p. 157 and 200.

(3) The two stones found in Lord Walpole's bladder were of this size, and weighed one of them 22 and the other 21 grains.

true, the stone may increase faster in some patients, and slower in others; but stones, after remaining a dozen or more years in the bladder, generally weigh several ounces. Some years since I saw a stone, weighing near six ounces, taken from a boy of no more than 14 years of age.

3. Lord Walpole's case not only shews the power of soap and lime-water to relieve the painful symptoms, and prevent the increase, of the stone in the bladder, but also makes it probable, that these medicines do communicate to the urine a power of dissolving the stone.

In the beginning of 1749 his Lordship voided with his urine a calculous substance of a flat shape, about the bigness of a silver penny, and covered with a soft white *mucus* (4); and upon the surfaces of the stones found in his bladder there were some inequalities, which seemed to have been made by the separation of thin *lamellæ* or scales. Further, the small stone found in the beginning of the *urethra* must have been in a dissolving state, and considerably lessened in the bulk: for, if it had lain long in the bladder, and never been larger, it ought to have been voided thro' the *urethra* with the urine; and it could not have arrived lately in the bladder, since Lord Walpole had not had, for several years before his death, any nephritic pains, or symptoms of stones passing from the kidneys; and since it is not likely, that a stone of the size and shape of the seed of an

(4) Philosoph. Transact. Vol. xlvii. p. 47.

apple (5) would pass thro' the ureters without being felt. Now if this small stone, found in the *urethra*, was partly dissolved by the virtue of the soap and lime-water; it will appear at least probable, that the two larger stones in the bladder were so likewise. But altho' Lord Walpole's calculous concretions had remained undiminished, and without any symptoms of dissolution; it would not therefore follow, that soap and lime-water cannot dissolve the stone in other patients, where the concretion may be of a less firm texture.

The Rev^d. Dr. Richard Newcome, now Lord Bishop of Llandaff, while drinking two English quarts of lime-water daily, for the cure of the stone in his bladder, poured his urine every morning and evening upon a piece of human *calculus* weighing 31 grains; by which, in the space of four months, it was reduced to three pieces, weighing in all only six grains. Upon one of these pieces, weighing 2.31 grains, he caused to be daily poured, for two months, the fresh urine of a person, who drank no lime-water; at the end of which time the piece of *calculus* was found to weigh 2.56 grains, having increased in weight a quarter of a grain. This same piece being afterwards steeped in the bishop's urine (who continued to drink lime-water as above), from June 24th to July 9th, was in these few days quite crumbled into powder. Since this experiment shews, beyond dispute, that lime-water, unassisted by soap, can communicate to the urine a power of dissolving the stone out of the

(5) The stone found in the beginning of the passage from the bladder was of this size, and weighed about a grain.

body, it can scarcely be doubted, that it must have the like effect on it, when lodged in the bladder. And that the dissolution of the stone in the bladder has been completed by soap alone, appeared evidently in the case of the Rev. Mr. Matthew Simson, Minister of Pancaitland near Edinburgh; an account of which will soon be made public (6) by Dr. Austin, who opened his body after death. Mr. Simson had, from 1730, been afflicted in a less or greater degree with the symptoms of a stone in the bladder; and in November 1735 was sounded by Dr. Drummond of Perth, and Mr. Balderston, surgeon in this city, by whom a stone was not only plainly felt, but also by the patient himself. In February 1737 he began to take soap; and after 1743 never had any gravelish symptoms. He died in May 1756; and, when his bladder was looked into, there was neither stone nor gravel found in it.

4. It appears from Lord Walpole's case, that soap and lime-water, even when taken in large quantities, proceed very slowly in dissolving the stone.

From July 1748, to the beginning of 1757, his Lordship drank three English pints of lime-water, and swallow'd for the most part an ounce of soap, daily; except from April 1750 to June 1751, during which time he took only one pint of lime-water, and one-third part of an ounce of soap, daily. However speedily soap and lime-water may dissolve the greatest part of urinary stones out of the body, yet

(6) It is printed in this volume of the Philosoph. Transactions, under the 28th of April, p. 221, & seqq.

being mixed with the aliment and humours of the stomach and guts, and afterwards with the whole mass of blood, it is impossible but their force must be greatly impaired before they arrive with the urine at the bladder. When, therefore, urinary stones are of an uncommon hard texture, we are perhaps scarcely to expect any sensible dissolution of them by the use of soap and lime-water: but when they are of a softer kind, there is no reason to doubt, that these medicines will in time dissolve them; and this will happen sooner or later, in proportion to the hardness of the stone, to the quantity of the medicines swallowed by the patient, and the exact regimen he observes, as to diet (7).

But however slowly soap and lime-water may proceed in dissolving the stone, yet they generally give speedy relief to the patient. Lord Walpole did not take these medicines in the full quantity till the end of July 1748; and, in a few months after, he was not only greatly relieved of all his complaints, but in December was able to ride an hundred miles in his coach, without finding any uneasiness, altho' the two last days of the journey the horses went at a full trot (8). In winter 1750, and spring 1751, when his Lordship swallowed only one third part of the soap and lime-water, which he had been in use to take, his pains and frequent inclination to make urine returned in a good degree (9); but, after taking the medicines in the full quantity, he soon became as easy as before (10).

(7) Essay on the Virtues of Lime-water, 2d edit. p. 140.

(8) Phil. Transact. Vol. xlvii. p. 46.

(9) Phil. Transact. Vol. xlvii. p. 472, 473.

(10) Essay on Lime-water, &c. p. 200, 201.

It would seem, while Lord Walpole used only one pint of lime-water and one third of an ounce of soap daily, that the petrifying quality of his urine was not intirely destroyed, and that the stony particles newly formed on the surface of the *calculi* occasioned, by their roughness, the return of his painful symptoms. However, when he had recourse to the medicines in a larger quantity, the petrescent quality of his urine was not only destroyed, but this fluid seems to have acquired a power of dissolving the rough stony particles deposited on the surface of the *calculi*; and in this way soon removed the pain, bloody urine, and frequent desire to make water, upon using any considerable exercise.

Soap and lime-water not only relieve the painful symptoms occasioned by the stone, by wearing off its sharp points, and rougher parts, which used to irritate the tender membrane, which lines the bladder; but, when this membrane has been wounded or lacerated by the stone, there is nothing, that will heal it more speedily than lime-water; which the ingenious Dr. Langrish has found to be remarkable also for its effects in curing the bladders of dogs, after being fretted with soap-lees (11).

The power of soap and lime-water to alleviate the painful symptoms attending the stone is so great, that, as far as I remember, I have only met with one patient, who did not find himself considerably relieved by them. But it is to be observed, that this patient neither took them in full quantity, nor persisted in their use for a long enough time: and, when he was afterwards cut, the stone taken out of his

bladder was almost as thick set with sharp prickles, as the back of an hedgehog: so that, in this case, no remarkable ease could be procured to the patient by the medicines, until they had quite dissolved these sharp points, and rendered the surface of the stone smooth and equal; which was not to be done but after a very long time, especially as the stone was of a pretty hard texture.

It may be proper to take notice, that when, along with the stone, there is any ulceration in the bladder, soap does mischief, and lime-water often fails of giving any considerable relief. However, even in this case it is perhaps one of the best remedies we know.

5. Soap and lime-water, taken in large quantities, and persisted in for many years together, appear to be innocent, and no way injurious to health:

Lord Walpole, who used these medicines for upwards of eight years, was not only relieved of the painful symptoms of the stone, but had his health improved by them in other respects (12). His appetite, healthful look, and a degree of spirits uncommon at his age, continued till the end of 1756, when his last illness began first to attack him. And as his health did not appear to be any way injured by these medicines; so, when his body was opened after death, his kidneys and ureters were observed to be quite sound and natural, as was likewise his bladder; only its coats appeared a little thicker than usual, owing probably to the long-continued friction of the stones upon it. Neither the kidneys, ureters, nor

(12) Essay on Lime-water, 6th edit. p. 171, 201.

bladder, were loaded or crusted with any calcarious matter; an effect most unjustly ascribed to soap and lime-water, since in the urinary passages, to which the air has no access, they cannot deposite their calcarious part (13); and since the white stuff observable in the urine of such patients, as take these medicines in large quantities, is only the usual sediment of the urine changed in its nature and colour, with, perhaps, some of the dissolved particles of the stone (14).

As the urinary passages were no-way injured, so neither were the stomach, guts, and other *viscera* of the lower belly. These had all a healthful appearance, except the gall-bladder, which was almost full of biliary concretions: nor is it surprising, that soap and lime-water, which prevent the growth of urinary *calculi*, should have no effect on biliary stones, since, altho' these medicines dissolve the former out of the body, yet they do not make the smallest impression on the latter.

I presume, it will be needless to take notice, that the lingering nervous fever, of which Lord Walpole died, cannot, with any colour of reason, be ascribed to the large use of soap and lime-water; since, if they could have produced such an effect, they must have done it in much less time than eight years and an half.

It may not be amiss to observe, that altho' soap and lime-water, taken in large quantities, are no-way injurious to health, yet in some cases they may

(13) Essay on Lime-water, &c. 2d edit. p. 170.

(14) Ibid. p. 24, 25. 30 & 31.

become improper, on account of the particular state of the patient. Thus, in a scorbutic or putrid disposition of the humours, soap at least ought to be totally omitted; and such patients, who are much troubled with the hæmorrhoids, ought to be sparing in its use, as the alkaline salt, with which it abounds, will scarcely fail to exasperate their pain. Where the patient is naturally very costive, less lime-water and more soap ought to be used; and, on the contrary, where the body is too loose, little or no soap is to be taken, but the cure is to be trusted to lime-water alone; which, in this case, ought to be drank to the quantity of two English quarts a day.

As the foregoing observations will, I am afraid, appear more tedious than important, I shall only add, that I am, with great esteem,

S I R,

Your most obedient humble Servant,

Robert Whytt.

III.

Dr. Pringle's Paper read after Dr. Whytt's Letter.

Read April 21, 1757. **D**R. Pringle begs leave to inform the Society, that having read the copy of his letter, within these few days, to Dr. Shaw, Mr. Hawkins, and Mr. Graham, those gentlemen found his account agreeable to their several observations; only Mr. Graham took notice, that, of late years, Lord Walpole, in his journies to Norfolk, had

twice voided some blood with his urine, but with little uneasiness; and that at other times he had passed some sand and stony particles (tho' never larger than the head of a small pin), attended with frettings of the parts, scarce painful. But Mr. Graham was not sure, whether these accidents were prior or subsequent to the sequel of the case, communicated to the Society by his Lordship.

Dr. Pringle thinks it may be likewise proper to acquaint the Society with another circumstance in Lord Walpole's case, which he had both from Dr. Shaw and Mr. Graham, *viz.* that after using the soap and lime-water for some time, his Lordship was freed from a very obstinate dry and scurfy eruption, which had resisted several other medicines. But as there were no marks of a putrid scurvy (that species expressly alluded to towards the end of Dr. Whytt's letter) the Society will easily understand, how the lithontriptic medicines may be prejudicial to one troubled with the true putrid scurvy (such as is most incident to sailors) and yet not be improper for those, that are subject to the scurfy eruptions, which are commonly, tho' erroneously, called scorbutic.

Pall Mall, 20 April.
1757.

XXVII. *An Account of the Virtues of Soap in dissolving the Stone, in the Case of the Rev. Mr. Matthew Simson. Communicated by John Pringle, M. D. F. R. S.*

To the Rev. Tho. Birch, D. D. F. R. S.

S I R,

Read April 28;
1757. **A** Few days ago I received from Dr. Austin, physician at Edinburgh, the case of the Rev. Mr. Simson, drawn up by himself, in the form of a letter to Dr. Austin; and which you may remember was alluded to by Dr. Whytt, in the paper read at the last meeting of the Society.

As I am at liberty to communicate this account to others, I thought it would not be unacceptable to the gentlemen of the Society, to have another well-attested instance laid before them of the virtues of soap in dissolving the stone, or, at least, in removing all those painful symptoms, which usually accompany that distemper.

To the patient's own narration I have subjoined an extract from Dr. Austin's letter to me, containing the sequel of the case from the date of Mr. Simson's letter to his death; with an account of the state of his bladder, as it appeared to Dr. Austin upon dissection. I am,

S I R;

Your most obedient humble Servant;

Full-Mall, 27 April,
1757;

John Pringle.

A. Let.

*A Letter from the Rev. Mr. Simson, Minister at
Pencaitland, to Dr. Adam Austin, Physician in
Edinburgh.*

Dear Sir,

Read April 28.

1757.

ACCORDING to your desire, I send you the history of my case; which is as follows:

I was of an healthy constitution till the year 1730, when I was seized with a frequent inclination to make water, without any previous pain in the kidneys or ureters. This symptom continued till the year 1733, without giving me much uneasiness.

In June 1733, as I was riding from Edinburgh to my own house at Pencaitland, I was seized with a great difficulty and pain in making water, which went off when I got home.

In the month of July, having again got on horseback, I was seized with the same complaint, but more violent; for then some drops of blood came away. From this time, if I rode eight or ten miles, I passed some blood, but without pain.

In September I made a journey of 60 miles on horseback; but every two miles was obliged to dismount, and made some bloody water.

I continued much in the same way all the year 1734, as the preceding; only had one additional complaint, of a pain in the glans after making water, and likewise in the neck of the bladder. The only thing I did for it was, to drink plentifully of warm milk and water; and gave over riding, on account of the bloody urine.

In

In the month of August I was founded by my nephew, Dr. Simson, professor of medicine in the university of St. Andrew's; but he found no stone, which he attributed to a wrong posture I was in, when he founded me.

During the winter, if I walked more than usual, I was sure to have a return of the bloody urine and strangury. . . .

In November 1735, I was founded by Mr. Balderstone, surgeon, in Edinburgh, a gentleman very expert in that operation, and likewise by Dr. Drummond of Perthshire. They both distinctly felt a stone: and I myself took hold of the catheter, when it was in my bladder, and felt the stone as distinctly, as if it had been in my hand.

About Christmas I was seized with a pain along the left ureter, and violent vomitings; but, upon using a turpentine clyster and opiates, it went off.

During the year 1736, I continued much the same as the preceding year, always drinking great plenty of milk and water, which gave me great relief, as to the bloody urine. . . .

I was advised by my nephew, Dr. Simson, to go to London, and be cut by Mr. Cheselden; the rest of my friends advising me to be cut by Mr. Smith, a lithotomist at Perth. However, I deferred the operation, and continued much the same all the year 1737, having severe fits now and then.

In the year 1738 Sir Alexander Gibson, of Addishton, informed me, that he had been in my condition, had passed several small stones, and had found incredible service from the use of soap pills: for, from not being able to get out of bed, in the space
of

of two months after using the soap he was able to go a hunting. However, for some time I was afraid to try the soap, not knowing what effects it might have on a confirmed stone; Sir Alexander Gibson's case being only that of small stones. But the Rev. Mr. Lundie, of Salton, by experiments convinced me of the efficacy of soap in dissolving a confirmed stone out of the bladder; for the stone gradually grew smoother and smoother, and at last was quite dissolved.

On the 12th of February 1739, I first began the use of the soap, and in the beginning took only a drachm in the 24 hours. The first week it made me a little qualmish: however, I gradually increased the dose; so that in six weeks I took six drachms a day, without its disagreeing in the least with me. I made it up into pills, and washed them down with a draught of warm milk and water.

From the time I began to use the soap, my gravelish symptoms gradually abated; but, upon walking two or three miles, I made bloody urine. However, that symptom gradually abated, and in the year 1743 all the symptoms of a stone quite vanished, insomuch that I could walk, ride, or go in a machine, as well as ever.

From February 1739, to July 1743, I took every day five or six drachms of soap: but after that time I diminished the dose to half an ounce; and never after had any return of a gravelish symptom, tho' I still imagine the stone is not intirely dissolved; for, after sitting some time, I find as it were something come to the neck of the bladder; but which gives me no uneasiness.

This,

This, Sir, according to the best of my memory, is my case : and if it can be of any benefit to you, in the cure of this painful disease, it will give great pleasure to,

Dear Sir,

Your most obedient Servant,

Nov. 20th, 1749.

Matthew Simson.

The Extract from Dr. Austin's Letter to Dr. Pringle.

Read April 28. ^{1757.} **T**HE Rev. Mr. Simson's letter to me was written in the year 1749; about which time he told me, that he had ridden 40 miles in a day, without any bad symptom ensuing.

In the year 1752 he broke his thigh-bone at the neck, by a fall from his horse, and continued for six weeks in great pain; but after that time he grew easier, and was able to put his foot to the ground. One day, as his servant was helping him to walk across the room, he let him fall; upon which Mr. Simson felt a severe pain: the broken leg became then evidently shorter than the other; and by that misfortune he was confined to his bed for near two years. However, about six months before he died, he was so well recovered, as to be able to go to church, and to perform divine service.

About the beginning of May 1756, Mr. Simson was seized with a diarrhæa, which resisted all medicine, and carried him off in the 83d year of his age. From the date of his letter to his death he had never discontinued the use of the soap (except during the time

of his last illness), tho' he had not been troubled with any painful symptom of a stone since the year 1743.

I obtained leave of his friends to open the body, but found no stone or gravel in the bladder; that part appearing to be, in every respect, in a natural state, except at the neck, where the coats seemed to be schirrous, and were about a quarter of an inch thick.

It is probable, that the stone had been of a softer texture, and more easily dissolved, than ordinary; otherwise five or six drachms of soap taken daily, even for so long a time, could not have dissolved it intirely; for many have used that medicine in much larger doses, and at the same time have drank lime-water plentifully, without obtaining such effects; tho' all their painful symptoms were removed by that course, as Dr. Whytt has shewn in his treatise on this subject.

I shall only add, that Mr. Simson's son, who is now minister at Fala, was present at the opening of the body, and can attest, that there was no stone found in the bladder.

Edinburgh,
15 April 1757.

Adam Autfin.

A Letter from Dr. Adam Drummond to Dr. Adam Autfin, relating to the Rev. Mr. Matthew Simson's Case. Communicated by J. Pringle, M.D. F.R.S.

Read June 23, 1757. **I** Have yours; and was present when Mr. Balderstone founded Mr. Simson; and both of us perceived, very distinctly, a large stone: and Mr. Simson himself felt it; which we were

were the more sollicitous he should do, as he was founded before by Dr. Simson, who had declared there was no stone. But the particular magnitude of it we could not well determine at the end of a long catheter; tho' I remember Mr. Balderstone, who was well versed in that business, conjectured it to be pretty large. He was founded only once by us, as the urethra was a little hurt by turning the catheter. There is only one circumstance in the case, which Mr. Simson seems to have omitted; that, from the first symptoms of the stone, he passed a great deal of *mucus* mixed with *pus*, as well as blood; and great quantities of gritty red sand, all in single grains, never any concreted into small stones. I take the more notice of this, as I do not remember, that, after he used the soap, he ever passed any sand, but a good deal of *mucus*, in which the soap was discoverable by its frothing. Could the gritty particles of sand be again suspended in the urine, so as to become invifible? or were they wrapt up in the soapy liquid, so as to escape observation? I have seen several stones of a soft consistence dissolved into mucilage by soap: but the sand passed by Mr. Simson, before he used the soap, seems to indicate his stone of a harder nature, tho' indeed it felt obtuse at the end of the catheter.

I shall rejoice, if many instances of this kind are found afterwards: but this seems to be the only one yet, of a stone in the bladder being dissolved by soap alone. I am,

Dear Doctor,

Bandeeran, June 5.
1757.

Your most humble Servant,

Adam Drummond.

XXVIII. *An Account of the Impressions of Plants on the Slates of Coals : In a Letter to the Right Honourable George Earl of Macclesfield, President of the R. S. from Mr. Emanuel Mendes da Costa, F. R. S.*

My Lord,

Read April 28, 1757. **I** Have the honour to address this letter to your Lordship, in order to be communicated to the Royal Society, if your Lordship deems it worthy the attention of that learned and illustrious assembly.

The impressions of various kinds of plants are frequently, I might say always, found in some of the strata lying over coal ; but more particularly in a stratum of earthy flat, which, in my History of Fossils, page 168. Species IV. I have synonymed *Schistus terrestris niger carbonarius*, and which always lies immediately upon the coal-stratum, not only in the coal-pits of this kingdom, but of many other parts of Europe, *e. g.* France, Saxony, Bohemia, Silesia, &c.

Most of these impressions, my Lord, are of the *herbæ capillares et affines*, the gramineous, and the reed tribes ; but, however, among them many rare and beautiful impressions undoubtedly of vegetable origin, and impressed by plants hitherto unknown to botanists, are not unfrequently met with.

Besides these, my Lord, found over coal-pits, there are likewise found in some parts of this kingdom, as at Robinhood's-bay in Yorkshire, Coalbrookdale in

Fig 2.

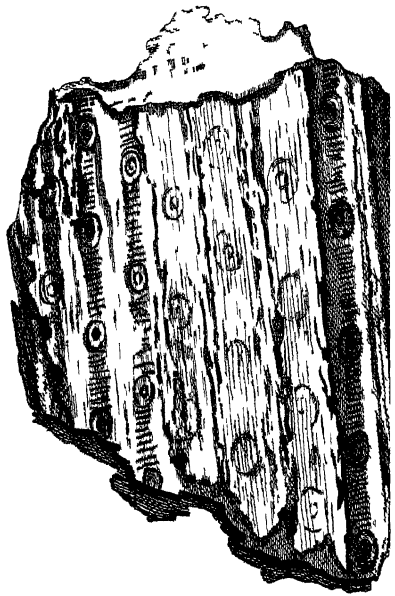


Fig 3

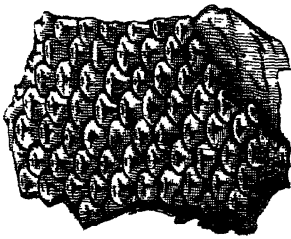


Fig 7.

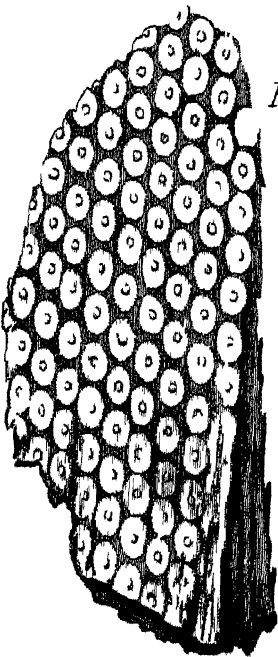


Fig 6

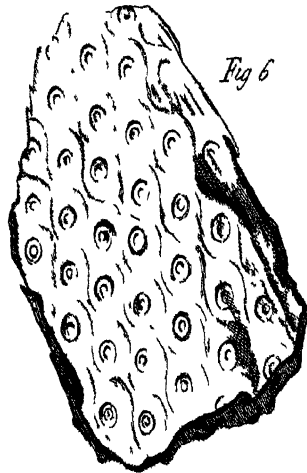


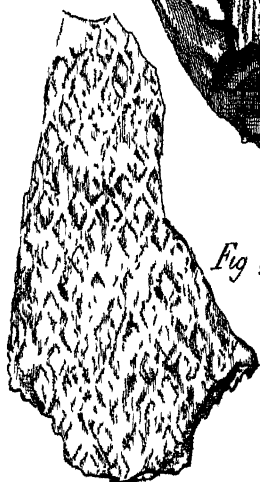
Fig. 1



Fig 4



Fig 5



in Shropshire, &c. many curious impressions of the fern tribe in regular nodules of iron-stone; and, in the latter place, not only impressions of plants, but even the cones or iuli of some kinds of trees are met with, very perfect and fair, and curiously imbedded in masses of iron-stone.

The most part of the impressions of ferns, grasses, &c. are easily recognizable, they so minutely tally to the plants they represent. Others indeed, tho' they do not exactly answer any known species, yet have characters so distinctly expressed, that they are easily arranged under their respective genera (1). Therefore I shall not trouble your Lordship with any further remarks on all such, but shall only touch on those elegant and extraordinary impressions, probably of unknown vegetables, above-mentioned: for that they are the parts and impressions of vegetables, I think clearly evinced, if we attentively and with a philosophical mind consider them, and reflect on the various circumstances, which attend them in the places, where they now lie buried.

I have therefore the honour, my Lord, to exhibit the drawings of seven such extraordinary impressions, and the fossils themselves, for your Lordship's and this learned body's inspection (See TAB. V.). The impression figured N^o. 1. is from Mr. Mytton's collection at Droit, near Oswestry, in Shropshire; as are N^o. 2, 4, and 7:

(1) The celebrated Dr. Scheuchzer has arranged the fossil plants botanically, by Tournefort's system, in his folio work, intitled, *Herbarium Diluvianum*, and Dr. Woodward's fossil plants, Catalogue B. he informs us, were botanically considered and arranged by those famous botanists Dr. Plukenet, and "Mr. Dood" Buddle, and Stenhouse.

sometimes two feet in length, and are generally covered with a thin crust of coal. The specimina Dr. Woodward exhibits, Catalogue B, pages 106, 107. specimina *q.* 22. and *q.* 32. are analogous to this, tho' not exactly the same. The Doctor's fossiles were from Haigh in Lancashire; and he imagines the impressions to be made by vegetables of the fir kind. Volckman also, in his *Silesia subterranea*, tab. 22. fig. 2. figures a branch with a rhomboidal work on it, and with three long narrow leaves, which seems akin to this impression.

N^o. 2. seems of the reed tribe: the knobs placed in rows, which are like the vesicles on the *quercus marina*, and some other *algæ*, are very remarkable. Woodward, Catalogue B. page 9. specimen *a.* 1. exhibits an impression akin to this, which he imagines to be of the fern kind.

N^o. 3. from a coal-pit in Yorkshire. I cannot but think this impression is owing to somewhat of the fir kind. Dr. Woodward, who exhibits such a like impression, Catalogue B. p. 16. specimen *a.* 108. imagines the same: his words are, "The impression is much like what might be made by the branches of the common fir, after the leaves are fallen or stript off."

N^o. 4. seems to be of the same kind as N^o. 2.

N^o. 5. This extraordinary impression is from Mostyn-colliery in Flintshire. It is a little obscured; but, when attentively viewed, exhibits a reticular impression, the meshes whereof are rhomboidal hollows, and the sides of the rhombs, or the net-work, are raised, or in relief.

N^o. 6. is from Newcastle. Volckman, *ibid.* part 3. tab. 4. fig. 9. seems to be of this kind.

N^o. 7.

Nº. 7. The same author, Volckman, figures a somewhat-like impression; *ibid.* fig. 5.

Only these seven extraordinary impressions I have presumed, my Lord, to treat of at this present time; but I have many more in my cabinet equally curious, some few of which I here exhibit to the Society, without taking any further notice of them: only I shall add, that many extraordinary impressions occur in Woodward's and other collections, and many are iconed in authors, worthy the attention of the curious.

These impressions, my Lord, are not only met with in small pieces; but large evident branches, some feet in length, have been found. I have, in the collieries of Derbyshire, frequently traced branches with (what seemed to me) long narrow leaves proceeding from them, and parts of other vegetables, above a foot's length: but the hardness of the substance they are immersed in renders it impossible to get them out without breaking them to pieces (2).

As these remains of vegetables are very extraordinary, I would recommend to the curious in botany to take notice of them, as an *Appendix Plantarum adhuc incognitarum*. For my part, I am so very little skilled in botany, that I hardly presume to offer my opinion; which is, that they are impressions and parts of species of the firs and pines, of the tithymals, the cereus's, and other arboresecent plants, and of large reeds; for some of the said

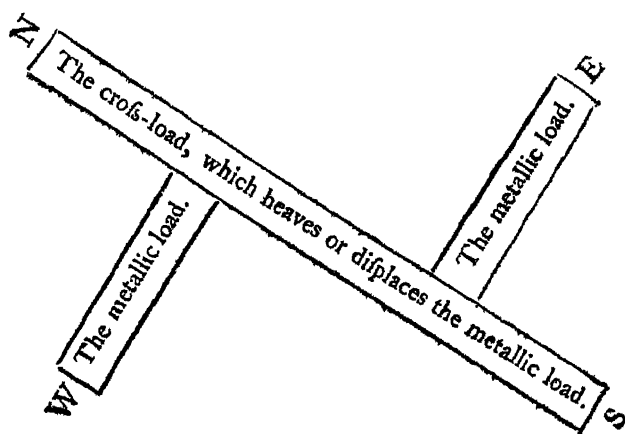
(2) Woodward, Catalogue B. p. 104. specimen *g.* 1. was of 6 1-half feet in length; and Catalogue D. p. 60. specimen *b.* 38. was a yard long; *et alibi passim*. In the collieries at Swanvich in Derbyshire, in 1752. a plant of the cane kind was found 14 feet long: it ended in a point at one end, and at the root in a large knob, and in the middle measured nine inches about.

kind are embellished with ribbed, studded, and reticulated works; *e. g.* the Hercules' club, or *rubi facie senticosa planta Lobelii*, described by Dr. Grew, *Museum Reg. Soc.* p. 221. the *cerei*, &c.

I further exhibit to the Society some few specimina of iron-stones with cones or iuli imbedded in them. These, my Lord, are from veins of ball iron-stone, in the lands of Lord Gower, at Okenyate, a village on the Roman road of Watling-street; and from the iron-works at Coalbrookdale in Shropshire. The cones are frequently met with in fragments, but rarely so intire, and are never found but in the strata of iron-stone. I have added to these a figured fossile body, much like a cone, found sometimes in our chalk-pits in England, but chiefly in the pits at Cherry-Hinton in Cambridgeshire. Dr. Woodward, Catalogue B. p. 22. specimen *b. 72.* calls them cones seeming to be of the *larix*; and imagines they were not come to ripeness or maturity. They certainly have some resemblance to cones, tho' I much doubt them to be so; but they most exactly resemble the roots of the *cyperus rotundus vulgaris* of botanists.

I shall finish this paper, my Lord, by acquainting your Lordship and the Society, that I firmly believe these bodies to be of the vegetable origin, buried in the strata of the earth at the time of the universal deluge recorded by Moses. It is, I must confess, with regret, that I find there are some, who reject the burial of these bodies at that fatal catastrophe, but substitute partial deluges to account for it. Did those gentlemen consider, or maturely weigh, the many remarkable and strong evidences of an universal deluge, every-where obvious in the bowels of the earth, they certainly would abandon their imaginary system:

system: for, my Lord, it is not only the immense quantities of marine remains, dispersed in all terrestrial strata, which are to be considered (that circumstance alone might give some reasoning to their system of partial deluges), but the following more weighty circumstances are likewise to be added and flung into the scale. 1°. The heavings, displacings, trappings, and breaks of the metallic veins, and the loads of rubble, met with at vast depths, and where no marine remains were ever found; and such heavings, &c. are not rare in metallic or mineral works: of which, to give your Lordship an idea, I have presumed to sketch the following plan of such a phenomenon.



These cross-loads are not unfrequent in the mines on North Downs, near Redruth, in Cornwall. Wheal-Widden copper-work there, in 1750, was about 60 fathom deep. The load was 20 feet over; and has many cross-loads two or three feet over, which sometimes heave the metallic load from one to five or six fathom. These cross-loads are generally filled with fragments of stone, minerals, and other rubble.

produced such effects. However, unprejudiced to any opinion, if the learned, who favour the system of partial deluges, will either confute these my assertions, or give solid reasons for the facts alleged to be producible by local deluges, atterrations, &c. I will joyfully embrace the truth : but till then, my Lord, I would recommend to those systematical gentlemen, not to pervert that excellent maxim of the great Lord Bacon, and, instead of *Non fingendum neque excogitandum, sed inveniendum quid natura faciat, aut ferat*, not to corrupt it into *fingendum atque excogitandum, non inveniendum quid natura faciat, aut ferat*.

I am, with great submission and respect,

MY LORD,

Your Lordship's

Most devoted,

London, 27 April,
1757.

and most obliged,

humble Servant,

Emanuel Mendes da Costa.

XXIX. *A Catalogue of the Fifty Plants from Chelsea Garden, presented to the Royal Society by the worshipful Company of Apothecaries, for the Year 1756, pursuant to the Direction of Sir Hans Sloane, Baronet, Med. Reg. & Soc. Reg. nuper Præses, by John Wilmer, M. D. clariss. Societatis Pharmaceut. Lond. Socius, Hort. Chels. Præfekt. & Prælector Botan.*

Read April 28, 1701 **A** Brotanum campestre incanum
1757. Carlinæ odore. C. B. P.

1702 Abrotanum humile corymbis majoribus aureis.
H. Reg. Par.

1703 Acer platanoides. Muntingii histor.

1704 Amelanchier. Lobel.

1705 Anchusa lutea minor. J. B. 3. 583.

Buglossum luteum annuum minimum. Tourn.
134.

1706 Arctotis ramis decumbentibus foliis lineari-lanceolatis rigidis subtus argenteis flore magno aureo pediculo longissimo. Miller's Icons.

1707 Ascyrum magno flore. C. B. 280.

1708 Asphodelus Allobrogicus magno flore Lili
H. L. 65.

1709 Aster caule ramoso scabro perenni foliis ovatis sessilibus pedunculis nudis unifloris. Miller's Icons.

1710 Astragalus repens minor flore cæruleo, filiqua Epiglottidi simili. Index Plant. Boerh.

- 1711 *Barleria inermis* foliis ovatis denticulatis petiolatis. Lin. Sp. Plant. 637.
Barleria folani folio flore coccineo. Plum. nov. gen. 31.
- 1712 *Blitum Kaly minus* album dictum. Kaly minus Ger. Emac. 535.
- 1713 *Campanula maxima* foliis latissimis. C. B. 94.
- 1714 *Caryophyllus montanus umbellatus* floribus variis, luteis ferrugineis Italicus. Barrel. obs. 648.
- 1715 *Cataria Hispanica Beticæ* folio angustiore flore cæruleo. Tourn.
- 1716 *Celastrus spinis nudis*, ramis teretibus, foliis acutis. Hort. Cliff. 72.
Lycium. Boerhaav. Ind. alt. 2. 237.
- 1717 *Cerasus racemosa sylvestris* fructu non eduli rubro. H. R. Par.
Cerasia racemosa rubra. 2. Tabernamont. Icon, 987.
- 1718 *Chamædrys Hispanica tenuifolia multiflora*. H. R. Par. Tourn. 205.
- 1719 *Cherophyllum palustre latifolium* flore albo. Boerh. 70.
Myrrhis palustre latifolia rubra. Tourn. 315.
- 1720 *Chenopodium Stramonii* folio. Vaill.
- 1721 *Cirsium foliis non hirsutis* floribus compactis. C. B. P. 377.
- 1722 *Cirsium maximum Asphodeli radice*. C. B. 377.
- 1723 *Colutea foliolis ovatis integerrimis* caule fruticoso. L. Phil. Miller's Icons.
- 1724 *Convallaria foliis sessilibus*, racemo terminali composito. Lin. Sp. Pl. 315.

- 1725 *Conyza mas Theophrasti major* Dioscoridis. C.
B. P. 265.
Major *Monspeliensis odorata*. J. B. 2. 1053.
- 1726 *Coriandrum majus*. C. B. 158. Officinar. 145.
- 1727 *Cornus Orientalis sylvestris fructu teretiformi*.
T. Cor.
- 1728 *Crithmum, five Fæniculum maritimum, minus*.
C. B. 288. Offic. 152.
- 1729 *Crocus sativus*. C. B. 65. Officinar. 152.
- 1730 *Cyclamen vernum minus orbiculato folio, in-*
ferne rubente, flore minore ruberrimo.
Mor. Hist. 3. 551.
- 1731 *Elichrysum graveolens acutifolium alato caule*.
Hort. Eltham.
- 1732 *Gramen spica aristata*. Lin. Sp. Pl. 83.
Gramen loliaceum spica longiore lolium Dios-
coridis. C. B. P.
- 1733 *Leucanthemum Tanaceti folio, flore majore*.
Boerh. 107.
Matricaria Tanaceti folio, flore majore, semine
umbilicato. Tourn. 493.
- 1734 *Meadia*. Catesby Hist. Car. 3. p. 1. Dodeca-
theon. Lin. Sp. Pl. 144.
- 1735 *Medica magno fructu, aculeis fursum et deor-*
sum rendentibus. Tourn. 411.
- 1736 *Moscattellina foliis fumarie bulbosæ, de qua*
Cordus. J. B. 2. 206.
Radix cava minima viridi flore. Ger. 933.
- 1737 *Narcissus Illyricus Liliaceus*. C. B. P. 55.
Pancratii Monspeliaci Hemerocallidis Valen-
tinae facie Lilio-narcissus, vel Narcissus ter-
tius Matthioli. J. B. 2. 613.

- 1738 *Osmunda vulgaris* et palustris. Tourn. 547.
Filix ramosa non dentata florida. C. B. P. 357.
- 1739 *Papaver laciniato folio capitulo hispido rotundiore*. Tourn.
Argemone capitulo rotundiore. Park. 369.
- 1740 *Papaver Orientale hirsutissimum magno flore*.
Tourn. Cor. 17.
- 1741 *Periclymenum perfoliatum Virginianum sempervirens et florens*. H. L. B.
- 1742 *Phillyrea, Oleæ Ephesiæ folio*. Pluk. Phyt. Tab. 310. fig. 5.
- 1743 *Phlomis Lychnitis*. Clus. Hist. 27.
Verbascum sylvestre Monspeliense flore luteo hiante. J. B. 3. 307.
- 1744 *Polygonum foliis cordatis caule volubili, floribus carinatis*. Lin. Sp. Plant. 364.
Fagopyrum scandens Americanum maximum.
Tourn. Inst.
- 1745 *Saxifraga sedi folio, flore albo, multiflora*.
T. 252.
Sedum Pyrenaicum pyramidale longifolium elegantissimum. Schol. Botan. Par.
Sanicula Pyrenaica longifolia multiflora elegantissima. H. L.
- 1746 *Serratula Noveboracensis maxima foliis longiferratis*. Hort. Eltham. 355.
- 1747 *Thalictrum minus*. C. B. P. 337.
Thalictrum minus seu Rutæ pratensis genus minus semine striato. J. B. 3. 487.
- 1748 *Verbascum fœmina flore luteo magno*. C. B. 239.
- 1749 *Vicia leguminibus sessilibus reflexis pilosis solitariis pentaspermis corollæ vexilis glabris*.
Lin. Sp. Pl. 736.

Vicia leguminibus folitariis deorsum flexis hirsutis. Sauv. Monspeliens. 235.

1750 Ulmus folio latissimo scabro. Ger. Emac. 1481.
Latiore folio. Park. 1404.

XXX. *Remarks on the Opinion of Henry
Eeles, Esq; concerning the Ascent of Va-
pour, published in the Philosoph. Transact.
Vol. xlix. Part i. p. 124. By Erasmus
Darwin, M.D. Communicated by Mr.
William Watson, F.R.S.*

To Mr. William Watson, F.R.S.

S I R,

THE inclosed papers were designed for the perusal of the Royal Society; being an endeavour to confute the opinion of Mr. Eeles about the ascent of vapours, published in the last volume of their Transactions. But the author, having no electrical friend, whose sagacity he could confide in, has at length prevailed upon himself to be so free to send them to Mr. Watson; to whom the world is so much indebted for the advancement of their knowlege in electricity.

Whence, Sir, if you should think, that these papers have truth, the great Diana of real philosophers, to patronize them, you will confer a favour upon me, by laying them before that learned Body. If, on the contrary, you should deem this confutation trifling
or

or futile, I hope you will be humane enough to suppress them, and give me your objections; and by that means lay a still greater obligation on one, who has not the pleasure to be personally acquainted with you. From,

S I R,

Your very humble Servant,

March 23. 1757.

Eraſmus Darwin,

Physician at Litchfield, Staffordshire.

L E T T E R I.

*To the very honourable and learned the PRESIDENT
and MEMBERS of the Royal Society.*

Gentlemen,

Read May 5. 1757. **T**HERE is ever such a charm attendant upon novelty, that be it in philosophy, medicine, or religion, the gazing world are too often led to adore, what they ought only to admire: whilst this vehemence of enthusiasm has generally soon rendered that object contemptible, that would otherwise have long laid claim to a more sober esteem. This was once the fate of chemistry: the vain and pompous boasts of her adepts brought the whole art into disrespect; and I should be' sorry, if her sister electricity should share the same misfortunes. It is hence the ingenious Mr. Eeles will excuse me, for endeavouring to lay before you my opinion on the ascent of vapours, tho' it by no means coincides with that he is so strenuous to establish, and plucks a plume from his idol goddess electricity.

The probability, supporting the hypothesis of Mr. Eeles, according to his own expressions, rests on this : “ That every particle of vapour is endued with a portion of electric fire ; and that there is no other sufficient cause assigned for their ascending.” (*Phil. Transf. vol. xlix. part. i. p. 134.*). My design is therefore first to attempt to shew, that another theory, founded on principles better known, will sufficiently explain the ascent of vapours : and then, that some kinds of vapours are not endued with a more or less than their natural share of electric æther.

The immense rarefaction of explosive bodies by heat, depends either on the escape of air before condensed in them, or on the expansion of the constituent parts of those bodies. This distinction has not been sufficiently considered by any one to my knowledge ; nor shall I at present amuse the Society upon this head ; it being enough for my present purpose to observe, that they may be thus distinguished : where air is emitted, it cannot be condensed again into the same bulk by cold ; but the expansion of heated parts of bodies, as soon as that heat is withdrawn, ceases to exist.

Nitre comes under the first of these classes : in detonation it emits great quantities of air, not afterwards condensable to the like space. This may be seen by firing a few grains of gunpowder in an unblown bladder, or in a vessel nearly full of water with its mouth inverted. The same is true of all the solid parts of animals and vegetables, when subjected to fire ; as appears from the experiments of that learned philanthropist, Dr. Hales.

But of water the contrary is evident. In the steam-engine,

engine, a jet of cold water, we find, instantly condenses that immense rarefaction ; which I apprehend could not be, if it was constituted of escaped elastic air. And altho' this steam must be acknowledged to put on some properties of air ; such as ventilating a fire ; or that a taper blown out by it, is capable of being again lighted immediately, and that without a crackling noise, which occurs when touched with water ; this does not in the least invalidate our opinion, tho' it has certainly conduced very much to propagate the former one : since from this way of reasoning, the whole must be air, and we should have no water at all in vapour.

From considering this power of expansion, which the constituent parts of some bodies acquire by heat ; and withal, that some bodies have a greater affinity to heat, that is, acquire it sooner and retain it longer than others ; which affinity appears from experiments, and which, I apprehend, is in some ratio of their specific gravities and their powers of refraction, reflexion, or absorption of light ; or at least in some ratio much greater than that of their specific gravities alone. From considering these, I say, many things, before utterly inexplicable, became easily understood by me. Such as, Why when bismuth and zinc are fused together, and set to cool, the zinc, which is specifically heavier, is found above the bismuth ? Why the buff covering of inflammatory blood, the skum of heated milk, the sedative salt of borax, which are all specifically heavier than the liquids in which they are formed, are still formed at the surface of them ? How benzoin, sulphur, and even the ponderous body mercury, may be raised into vapour, again to be condensed unaltered ?

And, lastly, how water, whose parts appear from the æolipile to be capable of immeasurable expansion, should by heat alone become specifically lighter than the common atmosphere, without having recourse to a shell inclosing air, or other assistant machinery? and when raised, I am persuaded we shall find, that to support them floating, perhaps many days, in the atmosphere, is not a knot so intricate, as to oblige us to conjure up a new divinity to unravel it.

But before we proceed to this second part of our task, it will be necessary previously to consider, first, how small a degree of heat is required to detach or raise the vapour of water from its parent-fluid. In the coldest day, I might say the coldest night, of winter, when the weather is not frosty or very damp, wet linen or paper will become dry in the course of a few hours. A greater degree of heat must indeed cause a quicker evaporation. But I am persuaded, that was it not for the pressure of the superincumbent fluid, greatly less than that of boiling water would instantly disperse the whole so heated into vapour.

Secondly, That in the opinion of Sir Isaac Newton, well illustrated by the late lamented Mr. Melvil, the sun-beams appear only to communicate heat to bodies by which they are refracted, reflected, or obstructed; whence, by their impulse, a reaction or vibration is caused in the parts of such impacted bodies.

This is supported by the experiment of approaching some light body, or blowing smoke near the focus of the largest glasses; and from observing, that these do not ascend, it is evident the air is not so much as warmed by the passage of those beams thro' it, yet would instantly calcine or vitrify every
opake

opaque body in nature. And from this we may collect, that transparent bodies are only heated at their surfaces, and that perhaps in proportion to their quantity of refraction : which will further give and receive illustration from those very curious experiments, of producing cold by the evaporation of liquors, published by the learned Dr. Cullen, in the late volume of *Essays Physical and Literary*, at Edinburgh. In these experiments a spirit-thermometer was immersed in spirit of wine, and being suddenly retracted, was again exposed to the air ; and as the spirit of wine adhering to the glass evaporated, the spirit contained within the thermometer was observed to subside. Now as the difference of the refraction of spirit of wine and glass is exceedingly minute, compared with the difference of refraction of spirit of wine and air ; we may consider, in the above experiment, the heat to be communicated to the thermometer only at its surface : but here the adherent fluid escapes as soon as heated ; by which means the glass, and its contents, are deprived of that constant addition of heat, which other bodies perpetually enjoy either from the sun-beams immediately, or from the emanations of other contiguous warmer bodies ; and must thence, in a few minutes, become colder than before.

The ingenious Mr. Eeles, I dare say, has already foreseen the use I am going to make of this principle ; viz. “ That the little spherules of vapour will thus, “ by refracting the solar rays, acquire a constant “ heat, tho’ the surrounding atmosphere remain “ cold.” And as from the minuteness of their diameters, if they are allowed to be globules, they must
do

do this to a very great degree, I apprehend none of those objections will take place against us, with which Mr. Eeles has so sensibly confuted the former received theories on this subject.

If we are asked, how clouds come to be supported in the absence of the sun? it must be remembered, that large masses of vapour must for a considerable time retain much of the heat they have acquired in the day; at the same time reflecting, how small a quantity of heat was necessary to raise them; and that doubtless even a less will be sufficient to support them, as from the diminished pressure of the atmosphere at a given height, a less power may be able to continue them in their present state of rarefaction; and, lastly, that clouds of particular shapes will be sustained or elevated by the motion they acquire from winds.

I should here have concluded this paper, perhaps already too long; but upon revising it, I find, where the affinity of some bodies with heat is mentioned, that the deductions made from thence are not sufficiently explained to be intelligible. First then, If the power of expansion of any two bodies, by heat, be in a greater proportion than their specific gravities, then will there be a certain degree of heat, in which their specific gravities will be equal; and another, in which the gravity of that, which was lighter when cold, will exceed the gravity of that, which was heavier when cold. Hence zinc and bismuth alter their specific gravities in fusion; some urine, and many solutions of solids, grow turbid as they cool; others alter their colours. Secondly, If (the power of expansion by heat being equal) the power of retaining

taining heat be in a greater ratio than the specific gravities; then, during the time of cooling after being sufficiently heated, there will be an instant, when the heavier body will become the lighter, and swim upon the other. This seems the case in the buff covering of inflamed blood, the skum of heated milk, and the cristallization of some salts: for if these effects were from the evaporation of the thinner parts at the surface, they should happen during the greatest evaporation, or when boiling; but, on the contrary, they are all done in the greatest degree when the liquor has for some time began to cool. Lastly, If the quickness of acquiring heat be in a greater proportion than their specific gravities (the power of expansion being equal), then, during the time of their acquiring heat, there will be an instant, when the body, that was heavier when cold, will now become the lighter. From one or more of which principles, I apprehend, the volatility or fixity of all minerals, and many other bodies, takes its origin.

It is no part of my design to account to you, gentlemen, in what manner such an expansion of the parts of bodies can be brought about by the action of fire. Tho' perhaps a rotatory motion only of each particle on its own center might be sufficient to produce such a rarefaction; and the more so, if such parts were any other figures than spheres, as by the percussion of their angles they must result further from each other. Nor is the existence of such a rotatory motion without some probability, when we observe the verticillary motion given to charcoal-dust thrown on nitre in fusion, or the wonderful agitation of the parts of burning phosphorus, or even of a
common

common red letter-wafer touched by the flame of a candle. But as in this paper I have laboured (and I hope not without success) to shew you, that some properties of solar heat are sufficient to account for the elevation and support of vapours ; so in another letter I propose nearly to demonstrate to you, that the electric æther is far from having any share in the production of this important phænomenon.

From,

GENTLEMEN,

Litchfield,
Mar. 20. 1757.

Your very humble Servant,

Erasmus Darwin.

L E T T E R II.

*To the very honourable and learned the PRESIDENT
and MEMBERS of the Royal Society.*

Gentlemen,

Read May 5, 1757. **E**VERY theoretical inquiry, whose basis does not rest upon experiments, is at once exploded in this well-thinking age ; where truth, under your patronage, has at length broke thro' those clouds, with which superstition, policy, or parade, had overwhelmed her. But experiments themselves, gentlemen, are not exempted from fallacy. A strong inventive faculty, a fine mechanic hand, a clear unbiaſſed judgment, are at once required for the contrivance, conduct, and application, of experiments ; and even where these are joined (such

(such is the condition of humanity!) error too frequently intrudes herself, and spoils the work.

My very respectable antagonist, Mr. Eeles, to whose ear, I am convinced, the voice of truth is more agreeable than that of applause, will forgive me the following critique on his performance; as by that means, I am persuaded, the probability of his notions will be intirely destroyed, and the foregoing theory receive additional supports.

For this purpose our first endeavour will be to shew the uncertainty of some of the most material principles, that support his arguments; and afterwards, the fallacy of the experiments he has given us.

First then, in page 130. Mr. Eeles has asserted, that the greatest possible rarefaction of water is when it boils. I think it might be said, with equal propriety, that the greatest rarefaction of solids was when they began to melt: and this may indeed be verbally true, if we chuse to alter the names of bodies, when they undergo any alteration by fire: so solids take the name of fluids, when they are in fusion; and water the name of vapour, when it is greatly rarefied in the steam-engine. Whence we find this assertion seems to be founded on a confusion in terms, and the fact far from being existent in nature.

In page 133. the sphere of electrical activity is said to be increased by heat. If by electrical activity is here meant an increase of its repulsive power (the thing, which seems to be wanted in Mr. Eeles's hypothesis), I know no experiment to show it. If it be meant, that it is capable of being attracted to a

greater distance ; I conjecture it may, as the heat will rarefy the ambient air, and we know the electric æther is attracted at very great distances in *vacuo* ; but this cannot properly be called an increased activity of electric fire.

We are afterwards told (page *ib.*) “ that electric fire will not mix with air : ” whence, in the succeeding section, it is argued, “ That as each particle of vapour ; with its surrounding electric fluid, will occupy a greater space than the same weight of air, they will ascend.” In answer to this, it must be observed, that there are some bodies, whose parts are fine enough to penetrate the pores of other bodies, without increasing their bulk ; or to pass thro’ them, without apparently moving or disturbing them. A certain proportion of alcohol of wine mixed with water, and of copper and tin in fusion, are instances of the first of these ; the existence and passage of light thro’ air, and, I am persuaded, of electric fire, are instances of the second.

To illustrate this, the following experiment was instituted. A glass tube, open at one end, and with a bulb at the other, had its bulb, and half way from thence to the aperture of the tube, coated on the inside with gilt paper. The tube was then inverted in a glass of oil of turpentine, which was placed on a cake of wax, and the tube kept in that perpendicular situation by a silk line from the ceiling of the room. The bulb was then warmed, so that, when it became cold, the turpentine rose about half-way up the tube. A bent wire then being introduced thro’ the oil into the air above, high electricity was given. The oil did not appear at all to subside :

whence I conclude, the electric atmosphere flowing round the wire and coating of the tube above the oil, did not displace the air, but existed in its pores.

This experiment I formerly tried various ways, as I had conceived, if the electric matter would displace air, it might have been applied to answer the end of steam in the steam-engine, and many other great mechanical purposes. But as from the above it appears, that the contrary is true, it is evident, that electric matter surrounding particles of vapour must, in fact, increase their specific gravity, and cannot any-ways be imagined to facilitate their ascent.

I may add further, that if this be true, that it pervades the pores of air, its specific levity cannot, by any means I know, be compared with that of air. Its particular attraction to some bodies, at least to much the greater part of the terraqueous globe, is abundantly greater than that of air to those bodies : and hence its gravitation to the whole globe would appear, at first view, to exceed that of air. But the more I consider this, the more perplexing and amazing it appears to me : and thence must leave it to the investigation of my very ingenious antagonist, or some other able philosopher.

I come now to the experiments, that are given us to show all vapour to be electrified. In these Mr. Eeles seems to have been led into error, by not having observed, that many bodies electrified will retain that electricity for some time, altho' in contact with conductors. The Leyden phial may be touched three or four times by a quick finger before the whole is discharged. Almost all light dry animal or vegetable substances, such as feathers and cork, do this in a

much greater degree: and in general I have observed, the more slow any bodies are to acquire electricity, the more avaritious they are to keep it.

Part of the plume of a feather, hanging to a green line of silk about a foot long, which was suspended from the midst of an horizontal line of the same, about four yards in length, was electrified with a dry wine-glass, according to the method of Mr. Eeles; and, after being touched nine times with my finger, at the intervals of two seconds of time, still manifested signs of electricity, by being attracted at the tenth approach of it.

A cork ball, on the same line and circumstances, after being electrified, was touched at the intervals of ten seconds repeatedly, for seven times, before it was exhausted. The fumes of boiling water were conveyed upon this ball after being electrified; and, after a fumigation for thirty seconds, it shewed signs of electricity, by being attracted to the approaching finger; and, after thirty seconds more, without any fumigation, it again obeyed the finger; and again, after thirty more, but at less and less distances. The same appearances occurred to me from the fumes of resin. From whence I apprehend, that Mr. Eeles, having dipped the electrified down of the *juncus bombycinus* in vapour for perhaps half a minute (for no time is mentioned), and finding it still retained its electric attraction, was not aware, that this same had happened, if he had by intervals touched it with his finger, or any other known conductor of electricity.

As Mr. Eeles had here objected, that there was no real opposition in the electric æther of glass, and that from wax; the common experiment to shew this was

was many times repeated with constant success; *viz.* the cork ball, suspended as above, after being electrified by the wine-glass, and repelled from it, was strongly attracted by a rubbed stick of sealing-wax; and *vice versa*. In the same manner I observed the electric æther from a black silk stocking (which was held horizontally extended by the top and foot, and, being rubbed in the midst with an iron poker, was applied to the cork ball), to be similar to that of glass, and opposite to that of wax. But the following experiment appears to me to put this matter out of all doubt, and to demonstrate, that this difference is only a *plus* and *minus* of the same specific æther, and not different qualities of it, as Mr. Eeles would suppose.

A stick of dry sealing-wax was rubbed on the side of a dry wine-glass, and a cork ball, suspended as in the former experiments, played for some time between them: but glass rubbed with glass, or wax with wax, did not manifest any electric appearance. Whence it would appear, that in rubbing glass and wax together, the glass accumulated on its surface the identical æther that the wax lost. Nor is this a digression from my design: for if this opposition of the electricity of glass and wax be established, it still contributes to demonstrate the fallacy of Mr. Eeles's experiments.

But what alone would intirely destroy this electric hypothesis, is, that from the experiments of Mr. Franklin and others, the clouds are sometimes found to be electrified *plus*, sometimes *minus*, and sometimes manifest no signs of electricity at all. Whence to say an accumulation of electric æther supports these
clouds,

clouds, seems an assertion built upon a very unstable foundation, whose whole superstructure may well enough be termed an air-built castle, the baseless fabric of a vision.

Add to this, that Mr. Eeles, in page 140. tells us, that himself has passed thro' clouds resting on the sides of mountains. Ought not those clouds to have immediately discharged their electricity, and fallen? And common experience may remind us, that any cold bodies will condense vapour, whatever be their electric properties. So mirrors, or the glass of windows, in damp rooms, are most frequently found covered with dew; which, of all other bodies, ought most to be exempted from collecting vapours supported by electricity, as they are the least capable to attract or draw off that æther.

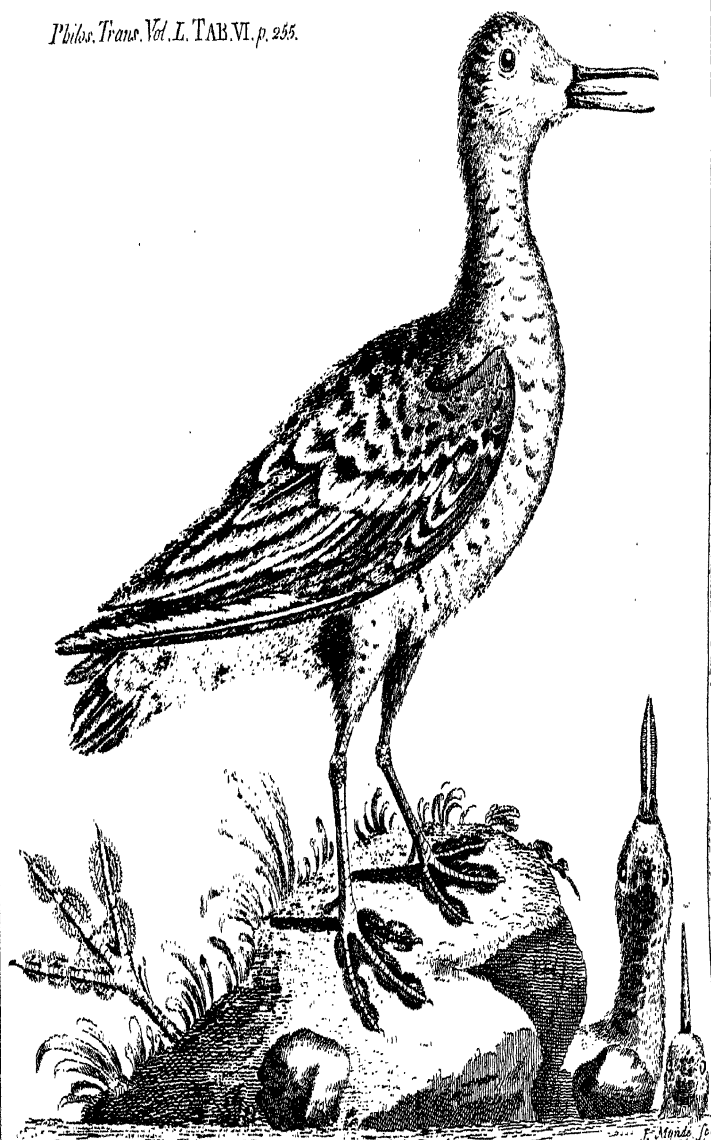
From all which, well examined, I am persuaded, gentlemen, you will be induced to conclude, that tho' clouds may sometimes possess an accumulation of electricity, yet that this is only an accidental circumstance, and not a constant one; and thence can have no possible influence either in the elevation or support of them. I am,

GENTLEMEN,

Your very humble Servant,

Litchfield,
March 23. 1757.

Erasmus Darwin.



The grey Coot-footed Tringa shot near Halifax in Yorkshire (January 1757) and Presented to me by Mr Thomas Bolton. Flights of Worley-clough in Yorkshire. Drawn from nature of the figures of Life by Geo. Edwards in Feb^y 1757.

XXXI. *An Account of a new-discovered Species of the Snipe or Tringa: In a Letter to the Rev. Tho. Birch, D. D. F. R. S. from Mr. George Edwards, Librarian of the College of Physicians.*

S I R,

Read May 5, ^{1757.} **I** TAKE the liberty to lay before you the figure and description of a new-discovered species of the snipe or tringa kind, which was lately shot at Sowerby-bridge in Yorkshire, and sent to me by Mr. Florist of Worley-clough, near Hallifax in the same county. If the account, that follows, shall be thought by you deserving to be communicated to the Royal Society, the real bird, which I have preserved dry, shall be produced at the same time.

This bird is like in shape to most others of the tringa or snipe kind. Its size is better shewn by the figure lying before you (*See TAB. VI.*), than by the dried bird, which is much shrunk since the drawing of it was made. I chuse, by way of distinction, to name it the coot-footed tringa, as it differs from other birds of that genus no otherwise, than in having its toes webbed in the same particular manner as the fulica, or our bald-coot. One of its feet is shewn in the plate, magnified a little, to make it the better understood, in what manner the webs or membranes spreading on both sides of the toes are scalloped or indented at each of the toe-joints. These scallopings are finely pectinated on their outer edges, as the enlarged figure expresses. The hinder toe is small, and
finely

XXXII. *Observationes de Corallinis, iisque infidentibus Polypis, aliisque Animalculis Marinis: Quas Regiæ Societati Londinensi offert Job Baſter, Med. Doct. Acad. Cæſar. Reg. Societ. Lond. & Scient. Holland. Socius.*

Read May 19.
1757.

DOMICILIUM meum mari propinquum † occasionem præbet, in nondum ſatis cognitam quorundam animalium, in mare degentium, generationem et œconomiam inquirendi. Quæ obſervavi Regiæ Societati temporis ſucceſſu offerre animus eſt; ſed in hac prima diſſertatione tantum obſervationes quædam de corallinis, iisque infidentibus polypis, et aliis animalculis marinis, exhibere in animum induxi.

Paucis abhinc annis, inter doctos viros diſſenſio fuit, utrum corallia, corallinæ, et kerato-phyta veræ eſſent plantæ, quæ creſcunt, et vegetant, in quibus inſecta marina nidificant; an vero horum ipſorum eſſent opus et fabrica. Multi et ſagaciſſimi hiſtoriæ naturalis ſcrutatores ultimam complexi ſunt ſententiam; ſed nemini contradicere ſtudens, ſimpliciter tantum et fideliter illa referam, quæ variis temporibus in corallinis obſervavi, et quæ lectorem benevolum, ut ſpero, convincent, corallinas non magis a polypis fabricari, quam diverſa fungorum genera ab illis fabri-

† Zirizææ, quæ, in inſula Scaldizæ, ſecundum Zelandiæ oppidum eſt,

cantur animalculis, quibus, æstivo tempore, quasi repleta inveniuntur.

Dura corallia, quæ recenter ex mari extracta, et in rotundum animalculis obfita reperiiebantur, primam ansam dedere suspicandi, hæc ab illis esse fabricata.

Cum omnia juniora conchyilia tenerrimæ et viscosæ substantiæ reperiantur, ex analogia conclusum fuit, parva et tenera animalcula, quæ coralliis infident, æque hæc fabricare potuisse, ac illa durissimas suas conchas et buccina. Sed in historia naturali non tuto ex analogia licet concludere.

Verum quidem est, quod recens natum conchylium tam tenerum est, quam parvus ille corallio infidens polypus; sed tunc concha ejus vel buccinum etiam erit tenerrimum, et quo magis in eo contentum crescit animal, eo major, durior et firmior fit concha: et interior conchæ superficies semper est lævissima et glaberrima, ne tenerrimum animalculi corpus aliquo modo lædi posset, ut in ostreis, mytulis, soleniis, et quibuscunque conchis et buccinis, hoc videre est.

Sed an hoc in coralliis invenitur? Nunquam polypi in ipsa coralliorum substantiâ habitant, sed semper intra hanc et circumdatam corticem. Cavitates, quas in coralliis invenes, non glabræ aut læves sunt, sed asperæ et acutæ. Parvus coralli ramulus nec tener est nec mollis, sed æque durus habita magnitudinis proportionem ac maximus: nec minores illi infident polypi quam ramis majoribus.

Cum vero hic, in Zelandia, necdum ulla dura corallia recenter ex mare extracta explorare licuit, me tantum ad corallinas determinare debui, ubique fere

prope littora reperiundas, et quibus simillimi, qui coralliis, infident polypi.

Animadvertam tantum mihi videri, animalcula, quæ summas coralliorum extremitates inhabitant, et Nobilissimo Marfiglio flores visa sunt, ad genus etiam polyporum referri debere, et in his extremitatibus non nata esse, sed irrepsisse, dum illas vacuas et domicilio aptas invenerint: eodem certe modo, quo cancelli, quos *Bernard Phermite* vocant Galli, vacua irreptant buccina. Et hi cancelli, ut observant piscatores, non casu aut temerarie id faciunt, sed quasi ex consulto. Si sex vel septem cancelli vivi prudenter ex suis buccinis extrahantur, et hæc inter se permixta iis iterum exhibeantur, quisque cancellus in proprium suum, nunquam in alterius buccinum irrepit, et hoc ablatum undique quæritat, quod jucundum visu est.

Necesse mihi fuit quasdam sed paucas corallinarum et polyporum figuras addere: si vero quis plura desideret, elaboratum opus sagacissimi Domini Ellis adeat, in quo quamplurimas et accuratissimas corallinarum et polyporum delineationes inveniet †.

De Plantis Marinis generatim.

Plantæ marinæ a terrestribus in plurimis differunt, nam hæ in raro aëre excrecentes, nutrimentum suum radicum ope, in terra proserpentium, hauriunt: dum illæ. plerumque nec radices nec folia habent,

† Dominus Ellis, anno, 1755. Lond. in 8^o edidit *Essay on the Natural History of Corals, &c.* quem librum Gallicè versum in 4^o recudit P. de Hond Hagæ Comitum, sub titulo *Essay sur l'Histoire Naturelle des Corallines, &c. par J. Ellis, 1756.*

sed ex trunco et ramis consistunt. Illa plantæ marinæ pars, qua substantiæ, cui increfcit, adhæret, radicis nomen non meretur, nisi quatenus plantam uni semper loco tenet adfixam : sed quod primum radicis munus est, nutrimentum ei non adfert : ipsa planta per truncum et ramos ex medio, cui semper immersa est, incrementum suum acquirit. Neque maris fundus radicibus recipiendis aptus esset, nam plerumque inconstans et volubile sabulum est, quod continuo fluctuum motu de loco in locum dimovetur, ita ut uno temporis momento radices nudæ, altero sub arena forent sepultæ.

Sed quamdiu vera vegetatio marinarum erit ignota, non bene explicari poterit, quare corallia et keratophyta, licet ab imo ad summum undique animalculorum cellulis obsessa, læte tamen crescant, ut hoc in plurimis, non tamen in omnibus, observare est. Nam Nobilissimus Marfigli keratophyta invenit, quæ nulla cortice, aut quæ uno loco cortice erant obducta, altero non : et corticem hanc non nisi polyporum cellulas fuisse clare ex ejus verbis patet *.

At rogare mihi liceat, an vera vegetatio plantarum terrestrium, quæ semper nobis ante oculos sunt, bene cognita et perspecta sit ? an quidem novimus, quæ vera sit radicum functio, et quomodo hanc exerceant ? Nonne plurimæ inveniuntur plantæ, quæ paucissimis instructæ radicibus in altum crescant, maxime ergo foliorum ope, quæ succos nutritios ex vaporibus in

* Il y a une sorte de Lithophyte, qui véritablement est curieuse, et bien extraordinaire : elle n'a point d'écorce continuée, mais bien quelques fragmens, par ci par là interrompus d'un glu, qui fleurit dans l'eau. Hist. de la Mer. pag. 89. fig. 101. 179, 1.

aëre natantibus hauriunt, plantam alunt, et sic radicem defectum suppleunt. Sed liceat mihi, accuratissimi Bonneti verba adferre; “ Plantæ, dicit hic sagacissimus naturæ scrutator, semper sunt fugentes, et in statu suctionis, interdiu radicum ope nutriuntur, noctu foliorum *. Sed optandum est †, quod arte quadam exacte posset determinari, et tunc inter se comparari hanc nutrimenti copiam, quam plantæ radicum ope acquirunt, cum illâ, quæ folia adferunt. Examen hoc forsitan nos doceret, quod *Aër* non minus quam *Terra* ad plantarum nutritionem et incrementum contribuat.”

Si ergo medium tam rarum et tenue, ut aër (ut ex Celi. Halæi et Boneti experimentis certum est) tantum ad plantarum nutritionem adferat, mirum non est, quod nunquam quiescens et quam maxime heterogenea aqua maris plantas marinas, licet expansis radicibus destitutas, ad tantam magnitudinem, altitudinem et duritiem faciat excrecere. Sed nonne similiter crescunt plurima fungorum genera? Quas radices habent quercubus aliisque lignis increscentes agarici? Quas phallus, elvela, et plura, quæ in *Methodo suo fungorum* describit J. G. Gleditsch.

Hæc de plantis marinis præmittere volui, ut evincam corallinas, licet radicibus careant, crescere, vegetare, et plantas esse posse, ut aliæ terrestres et fungi, quæ similiter aut minimas habent radices, aut plane iis destituuntur,

Cætera, quæ de vegetatione, floribus et seminibus plantarum marinarum observavi, alio tempore indicabo.

* Bonnet sur l'usage des Feuilles, pag. xviii. & 286.

† Id. ibid. pag. 66.

De Corallinis.

Corallinæ omnes habent proprietates, quas in genere de plantis marinis indicavimus: Sed præterea, quod præcipuum est hujus differtationis propositum, omni fere corallinæ, si rami ejus ad justam magnitudinem et firmitatem creverint, tam hyeme quam ætate, animalcula infident, quæ a multitudine brachiorum, et similitudine, quam cum polypis, in aqua fossarum dulci, reperiundis, habent, etiam polypi vocantur.

Si quis corallinæ plantam, eique infidentes polypos, rite examinare studet, non incipiat parvam tenuis ramuli partem in vitro concavo jacentem microscopio inspicere; sed totam corallinæ plantam recentem ex mare extractam in vitrum pellucidum, aqua marina repletum, prudenter inferat: aut talem plantam orbi porcellano concavo in fundo coloris profunde cærulei imponat, et addita sufficiente aquæ marinæ pellucidæ quantitate, ejus ramos prudenter expandat; tunc post quadrantis horæ quietem, illos lente amplificante intueatur, et sic distinctissime in hac unica planta polypos diversi generis, et plerumque mira alia videbit infecta; quæ microscopio dein ad libitum ulterius examinari possunt.

Corallinæ, quæ ^a capillares et filamentosos habent ramulos, aut quæ ^b juniores et tenuiores adhuc sunt, ut

Quæ navibus post longum iter;

Aut quæ doliis istis coniformibus, quæ ad littora vel fluminum majorum exitum in mare ad nautarum securitatem ponuntur;

^a Tab. VII. fig. I, II.

^b Ibid. fig. III.

Aut januis emissariorum aquæ marinæ in his regionibus,
accreverint, raro vel nunquam habent polypos.

Sed si eadem corallinæ species jam ad sufficientem magnitudinem et firmitatem, et præsertim supra ostrea, saxa, filices, aliaque in fundo maris quiete jacentia corpora, increverit, polypis scatet. Perspicaci suo judicio decidat lector B.

An hoc efficitur, quia polyporum semen, ova, vel nata progenies gravitate sua fundum petat?

Vel quia animalculis his perpetuus navium aut doli-
lorum motus obstet, ut hæc non satis tuta credant,
et ideo in fundo maris semper immota eligant corpora,
quibus ovula sua confidunt?

Vel quia pix et colophonias, quibus naves, dolia, et
emissariorum januæ illiminuntur, corallinis, quæ illis
increscunt, noxiam vel polypis ingratam qualitatem
communicent? Hoc saltem semper obvenit, me
numquam tot polypos invenisse in corallinis, quas a
navibus, doliis aut emissariorum januis abraferam,
quam in illis, quæ ostreis, mytilis et filicibus in fundo
maris erant innatæ.

Hoc *primum* mihi *argumentum* est, corallinas a
polypis non formatas esse; nam tunc plantæ juniores
et minores æque suos haberent polypos, ac maximæ.

Secundum argumentum, quod polypi vel casu vel
instinctu quodam sese corallinis affigant, sed veram
plantæ partem non constituent, est, quod polypi non
omnes majoris plantæ ramos æqualiter obsident: hic
ramus vel hujus rami tantum pars polypis obsessa erit
quam plurimis, altera nullis. Corallinam habeo, c

cujus truncum plurimi inhabitant polypi, dum nullos in ramulis poteram detegere, licet armato oculo. Et sic algæ * vel quercui sic dictæ marinæ sæpius corallinæ increscunt, in quibus nunquam polypos inveni. Hoc fieri non deberet, si corallinæ polyporum essent opus. Omnes corallinæ semper suos deberent habere, et ramis suis æqualiter et proportionaliter insidentes polypos; et nunquam sine his essent reperiundæ, ut tamen sæpius fit. Cel. Jussieu † quasi mirabundus dicit, se semel alcyonium et spongiam ramosam sine polypis invenisse, licet recenter a rupe essent abstractæ.

Tertium argumentum erit, quod fere semper una eademque ^d corallinæ planta diversi generis alat polypis: in una eademque ^e corallinæ tubulariæ planta quinque diversas polyporum species inveni ‡.

Liceat jam mihi rogare, quibusnam horum quinque hæc corallina ortum suum debeat? Certe non primæ aut secundæ, ut videtur, magnitudinis, nam hi summis tantum insident corallinæ extremitatibus, et microscopium clare ostendit locum, ubi corpus polypi minus pellucidum et superficiei rudioris glabræ huic corallinæ adhæret. Nec fabricavit corallinam tertia species, quæ extrema corporis parte, quasi caudâ, corallinæ est affixa: et minus adhuc quarta, nam clare videre est, horum cellulas corallinæ cir-

^d Tab. VIII. fig. II, IV.

^e Tab. VIII. fig. IV.

* Vide talem delineatam in Mem. de l'Acad. p. 394. anni 1711.

† Mem. de l'Academie, 1742.

‡ Sic D^s. Ellis in una eademque planta diversos polypos delineat, tab. IV. fig. C. tab. V. fig. A. tab. XIV. fig. A. B. tab. XXXVIII. F. N. E.

eumdatas, ut examen apum arboris ramum circumcludit: et si hæ polyporum cellulæ non nimis densæ sunt, ipsius corallinæ color translucescit.

Si hæ quatuor polyporum species hanc corallinam non fabricaverint, non fecit certe quinta. † Mirabilissima et minima hæc animalcula ad genus polyporum certe pertinent, et omnibus fere corallinis, algis, aliisque plantis marinis incredibili sæpe infident copia. Jucundissimum est videre, quomodo se expandunt, et mox mira agilitate, capta prædâ, se subito contrahunt, quod bis vel ter in minuto horæ repetunt.

Æque jam, ut in una eademque corallinæ planta diversos invenis polypos, sic in diversis corallinæ speciebus videbis similes et eosdem polypos: * quod etiam illi obstat sententiæ corallinas polyporum esse opus aut fabricam. Polypi dum operantur, ut cætera animalia, instinctu innato operantur: ergo, una eademque polyporum species semper eandem fabricaret corallinas: sic semper similes et uniformes apes faciunt favos, vesipæ nidos, araneæ tela. Hic vero contrarium eveniret; iidem polypi uno tempore hanc, alio tempore illam fabricarent corallinam: quod rationi contrarium est, et mihi *quartum argumentum*. Sed hoc verum est, quod eadem polyporum species non diversas corallinas, sed in diversis corallinis eandem et uniformes semper sibi construant cellulas.

Quinto, si corallinæ a polypis essent fabrefactæ, nunquam polypi et eorum cellulæ etiam vivis animalibus, aliisque corporibus, essent adfixæ. Polypos inveni in

† Tab. IX. fig. A, B, C.

* Sic Clar. Ellis eosdem etiam polypos invenit in corallina astaci cornicularum æmula N^o. 14. et in corallina setacea instar arundinis geniculata N^o. 16.

ostreorum

ostreorum g conchis, cancri ^h arachnoides pedibus, animali i, quod emissariorum januis et navibus quietis sæpius adhæret et anus (*aars-gat*) vocatur, et aliis plurimis, sine minimo corallinæ vestigio. Et animalia illa, quæ piscatores nostri k *klap-konten* vocant, et majorum polyporum species esse videntur, nunquam teneris insident corallinis, sed semper hæc supra ostreorum conchas et lapides inveni, et quamvis illorum progressum ipse non viderim, tamen de loco in locum sese transmovisse, expertus sum.

Sexto: Hæ corallinæ non solum polypis, sed cochleis, buccinis ^l aliisque plurimis insectis marinis conveniunt, ut illis ova vel progeniem confidant. Hoc præcipue mensibus Februario et Martio videre est: accepi tunc diversas corallinas diversis cochleis et buccinis plenas, et sæpe inter hæc quosdam cancellos ova sua jamjam excludentes. Mater cochlea ovula sua supra vel juxta corallinas deposuit; pulli exclusi contra has ascenderunt, ne illis innixi fluctuum motu eluerentur, vel ut adversus hostes suos tuto se absconderent.

Quantum omnibus animalibus divina prospexerit prudentia, animus nunquam satis assequi quacunque industria potest. *Deus*, ut jam animadvertit Rex † Psalmista, *plantavit arbores cedros Libani, ubi aviculæ nidifcent, et abietes domicilia ciconiæ: Montes excelsissimos rupicaprarum, petras murium montanorum perfugium*. Sic corallinæ domicilia et perfu-

g Tab. VII. fig. VI. ^h Ibid. fig. VII. ⁱ Ibid. fig. VIII.
 k Tab. IX. fig. IV, V, VI. ^l Tab. VIII. fig. VI.

† Psalm. civ. vers. 16, 17, 18.

gium sunt polypis aliisque minoribus insectis marinis. Et præsertim hoc censeo, quia tempore hyemali, mensibus Decembri et Januario, corallinarum ramos plurimis vesiculis, operculo vel valvula tectis, obfessos inveni: quare has vesiculas habeo pro ovis ab aliis insectis his corallinis impositis.

Omnes, qui nunquam satis laudanda incomparabilis REAUMURII scripta legerunt, sciunt, quam miris et differentibus modis insecta quædam sua ova opponunt, vel in * gyrum, vel in † lineam spiralem, vel ‡ singula ova a filis libere in aere pendentia, vel, ut hic fere casus est, per || paria lignosæ substantiæ ramorum imposita. Quam mira quam varia est etiam ipsorum :‡: ovorum figura! Culicum † autem ova, quoad externam figuram, quam maxime his corallinæ vesiculis similia sunt.

Omnia vero insectorum ova inter se et cum his vesiculis in eo conveniunt, quod :: operculum vel valvulum habent eo in loco, ubi eruca vel vermis exitum invenire debet; permittente quidem hac valvulâ exitum extrorsum, omnem vero introitum aëris vel aquæ prorsus negante. Sed præter hanc cum aliorum insectorum ovis convenientiam, ipsæ vesiculæ mihi vera ova videntur, quia recenter ex mare extractæ erant perspicuæ, in spiritu frumenti vero statim fie-

* *Memoir. des Insect. tom. ii. tab. iv. fig. 6, 8.*

† *Id. ibid. tab. iii. fig. xv.*

‡ *Id. tom. iii. mem. xi. tab. xxxii. fig. 1.*

|| *Id. tom. v. mem. iii. tab. xv. fig. 1, 2, 3.*

:‡: *Id. tom. ii. mem. ii. tab. iii. tom. iv. tab. xxxvii. fig. 11, 12, 19, 20.*

† *Id. tom. iv. tab. xlv. fig. 8.*

:: *Id. tom. ii. mem. ii. pag. 163. tab. iv. fig. 11, 12, 13. Ellis Essay, pag. 100. tab. xxxiii. A.*

bant opacæ; albæ, cum puncto flavo m. Secundo, quia hyeme plures vesiculæ in corallinis reperiuntur, perpaucae vero æstate; nam tum illorum pulli exclusi sunt, et corallinae cochleis parvis, forsan ex his ovis progenitis, magis sunt obfessæ.

Nam hæ vesiculæ mihi ipsorum polyporum ova esse non videntur, licet sæpius * polypos in illis invenire contigit: et ideo crederem hos polypos in vesiculis repertos, quoniam licet adulti tamen vesiculis multo minores sint, post veri pulli exclusionem in vacua illa ova irrepsisse; cum jam antea observavimus cancellos in vacua irrepere buccina, et ipsos polypos vacuis corallinae tubulariæ summitatibus sese adfigere. Sed quam maxime has vesiculas non polyporum, sed aliorum animalculorum ova esse censeo, quia perspicacissimus *Reaumurius* initio hyemis in oris Galliæ provinciæ *Pictaviensis* (*Poitou*) ova cochlearum marinarum invenerit, quæ quamvis his vesiculis paulum majora, erant tamen simillima †. Licet corallinae speciosam habeant verisimilitudinem, ut quasi animalium opus appareant, aliæ tamen sunt plantæ marinæ, præsertim *alcyonium digitatem molle*, quas adhuc magis a polypis fabrefactas diceret; sed si accuratione indagine observantur, pro veris plantis erunt agnoscendæ, ut in posterum, quando profusius agam de plantis marinis, demonstrare spero. Alia vero alcyonia, ut *vesicaria marina Baubini*, &c. vera animalium opera, namque eorum sunt ovaria. Plantæ marinæ unicæ non sunt, quæ pro insectorum opere

m Tab. VIII. fig. VI. a A.

* Ellis passim in figuris, præsertim tab. v. fig. A.

† Mem. de l'Acad. viii. pag. 253. tab. vi. fig. 1, 2, 3, 4.

sunt

sunt habitæ. Liceat mihi locum adferre ex notis, quas Nobilissimus *Lyonnet Theologiæ insectorum Doctiss.* et *Rev. Lessers* subjunxit: “Credendum non est, ut
 “quidam faciunt, si stagnantes aquas viridi et fibrosâ
 “membrana tectas videas, hanc insectorum textu-
 “ram esse. Est algæ species, quæ in his stagnis
 “crescit, et infectis est pro alimento.” *

De Polypis Corallinis insidentibus.

Completam historiæ et oeconomiae polyporum in corallinis repertorum descriptionem dare nondum suscipio, sed paucis enarrabo, quæ de illis observare mihi licuit.

Auctores, qui aquæ dulcis polypos examinarunt, quatuor eorum species enumerant: sed in aqua marina major differentium specierum numerus est; plus quam viginti diversos vidi polypos, quos ut plurimum in laudatis figuris etiam delineavit accuratissimus *Ellis*, et profecto adhuc plures sunt. Sed hic monendum, quod cum aquæ dulcis polypi nudo oculo facile queant conspici, plerique marini non nisi lente aut microscopio possint examinari: et aqua marina, in qua polypi vivunt, bis, vel ad minimum semel, nucthemeri spatio, debet renovari, aut polypi moriuntur.

Ad exemplum *Di. Schaffer* in duas species primo polypos distinguam; in polypos, qui cellulas sibi construant, et in polypos, qui corporis extremitate corallinis aliisque corporibus extus sese affigant, vel in cavitates naturales alcyoniorum, coralliorum, &c.

* *Lesser*, *Theologie des Insect.* tom. ii. p. 112.

(de quibus in posterum) irrepan, ut cancelli in vacua buccina. Polypi, qui in cellulis habitant, cæteris plerumque sunt minores: horum cellulæ corallinæⁿ truncum vel ramos circumdant, a quibus prudenti manu sine corallinæ læsione satis facile possunt abradi: et si hæ cellulæ non nimia sint copia, et corallina contra solis splendorem inspiciatur, præsertim si color vivide ruber, aureus aut subniger est, facile per ipsas cellulas transluet.

Polyporum, qui sine cellulis in corporum quorundam cavitatibus habitant, aut extrinsecus illis affixi sunt, plurimæ sunt species. Apertis^o corallinæ tubulariæ ramis (nam in tali planta semper adsunt rami vel tubi naturaliter^p clausi, id est, integri, in quibus nunquam polypus infidet) majorum polyporum species sæpius supra sedet; quos a colore rubente polypos vocabo *coccineos*, et præ cæteris observavi. In detritis et apertis, ut modo dixi, corallinæ tubulariæ ramulis, inserta sua cauda vel corporis parte posteriore, polypus habitat, et ex hoc ramo facile et sæpe levi quassatione delabitur, præsertim si ille jam per aliquot dies servatus debilis, æger, aut mortuus sit †.

Si vegetum talem polypum, corallinæ infidentem, vehementius amplificante microscopio intueris, facile differentiam vides inter scabram et quasi granulatam polypi cutem, et lævissimam corallinæ corticem.

Hic polypus coccineus duplici brachiorum genere est munitus, quod in aliis minoribus non vidi; et hæc

ⁿ Tab. VIII. fig. II. IV. ^o Tab. VIII. fig. III. ^p Tab. VII. fig. III.

† Idem observat Cel. Jussæus, licet corallinas a polypis tamen fabricatas autumat. Vide Mem. de l'Acad. 1742. et figuram in spice, quomodo corporis extremitate corallinæ infident.

coloris sunt subalbidi. ^a Inferiora et longiora octo-
decim vel viginti sunt, nam numerus sæpe variat, et
expansa patinam formant, in cujus medio ^r ipsum
polypi corpus coccineum est. Hoc corpus in duas
partes dividi potest. Inferiori placentam referenti per-
pendiculariter alia pyriformis est infixa, quæ duodecim
^s vel quatuordecim habet brachia, prioribus tenuiora
et breviora.

Hanc partem pyriformem polypus valde ^t extendere
potest, præsertim si prædam captans expansa clau-
dit ^u brachia; et rursus ita contrahere, ut quasi ^x glo-
bulus inferiori et depressæ parti adhæreat. In extremo
hujus partis ^y os polypi esse videtur: sed ob partium
exilitatem non omnia satis distincta possunt videri, ut
in majoribus aquæ dulcis polypis; sed ex similitudine
partium hæc tuto licet concludere.

Si longiora brachia fortiori microscopio attente con-
templaris, cutem eorum valde asperam vides, ut est
piscium (quos *Haijen* vocant) vel ut superficies corii
granulati (*chagrein-leer*) forsitan ut minora animal-
cula, quæ polyporum esca sunt, eo melius, ne ela-
bantur, retinere possint. Sed inter hujus speciei po-
lypos vidi unum cæteris ^z majorem, ex cujus cor-
pore, illo loco ubi pars superior inferiori et depressæ
inhæret, sex vel octo enascebantur ^a ramuli, in quo-
rum extremitatibus erant duo vel tres parvi globuli,
punctum coccineum in medio habentes. Huic po-
lypo bis in die novam dedi aquam, quam maxime de
ejus vita sollicitus, ea spe, ut hi globuli in juniores

^a Tab. IX. fig. II. *a* A. ^r Tab. IX. fig. II. A. *c.* ^s Ibid. *b. b.*
^t Ibid. A. *c.* ^u Ibid. B. ^x Ibid. C. *c.* ^y Ibid. fig. III.
^z Ibid. fig. III. ^a Ibid. *c. c.*

excreverent

Excrescerent polypos, sed quamvis per mensem sic vivum servaverim, nihil mutatum vidi, nisi quod globuli paululum evaserint majores.

Quæ deinceps de polyporum generatione observavi, non ita mihi satisfaciunt, ut illa pro certis et comprobatis habeam: si vero hac æstate meliora adiscam, Regiæ Societati indicabo.

De aliis Insectis Marinis.

Si noctu aqua maris, quæ littora nostra alluit, lapide injecto vel baculo movetur, innumeras videre est scintillas igneas, quæ nihil aliud sunt, quam minima animalcula lucentia, non nisi fortiore microscopio visibilia.

Ut hæc animalcula idoneâ colligas copiâ, sufficientem aquæ quantitatem, in qua has scintillas observasti, per chartam emporeticam filtrare facias, donec semuncia aut minus aquæ supra chartam restat: hujus guttula, vitro concavo, penicilli vel pennæ ope, imposita, fortiore microscopio examinetur, et celerimo motu illa natare videbis. Tres diversas horum observavi species, quas ad vivum delineatas exhibet tabulæ X. fig. I.

Sed mare plura alit insecta, quibus hæc lucendi facultas inest, et quorum in corallinis repertorum quædam delineata sunt tab. X. fig. 1, 2, 4, 5. sed quoniam plures † auctores de his scripserunt, non commemorabo.

Si corallinæ recenter ex mare extractæ major planta, orbi porcellano concavo, et cujus fundus est

† *Vionelli* nuove luci coperte. *Linnaei* Amæn. Acad. tom. iii. de noctiluca marina.

profundi coloris ~~æ~~arulei, cum sufficiente quantitate aquæ marinæ pellucidissimæ et filtratæ, ut ante jam monui, imponatur, et ejus ramuli prudenter penna expandentur, et lente oculari inspiciantur, silvam sæpius videre est, in qua plurima pascuntur animalia, præter diversi generis polypos ramis infixos et brachia extendentes: alia plura sunt, præsertim in infima parte, si corallinæ supra ostrea creverint, quæ huc et illuc cursitant, et forsitan sæpius hianti ostreo alimento inserviunt. Sic vigesimo tertio Octobris 1756 ostreum accepi, cui magna ^b corallinæ planta erat innata, in qua, præter tres diversas polyporum species, sex differentia reperiẽbam insecta. ^c Primum vermis erat, cujus caput sex majoribus et duobus minoribus cornubus erat instructum. ^d Alterum valde parvum araneam longipedem (Gallis *le Faucheur*) referebat, admodum lente se movens. Tertium vermis erat, similis figuræ 3, sed in designando deperdebatur. ^e Quartum, quintum et sextum non nisi fortiore microscopio distincte erant visibilia. Horum quod littera c notatur, mirandæ erat structuræ.

Diversa sic ostrea et corallinas explorans, plura talia admiranda insecta vidi, quorum delineationem sistunt tabulæ X. figuræ 2, 3, 4, 5, 7. Decimo sexto ejusdem mensis Octobris, plures mihi sed valde parvæ apportabantur corallinæ, quæ a dolio coniformi erant abrasæ: in his licet sæpe et attente exploratis nullos detegere poteram polypos, sed duo alia mirabilissima insecta.

Horum, quæ secunda figura tabulæ decimæ littera A.

^b Tab. VIII. fig. VII.

^c Tab. X. fig. I.

^d Ibid. fig. 6.

^e Ibid. fig. 8.

Fig. I.

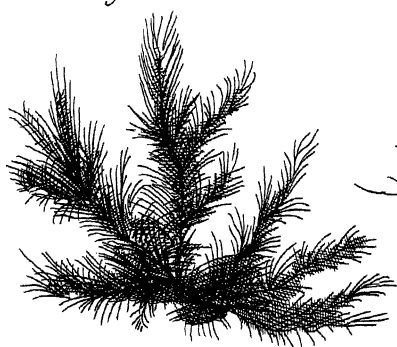


Fig. II.



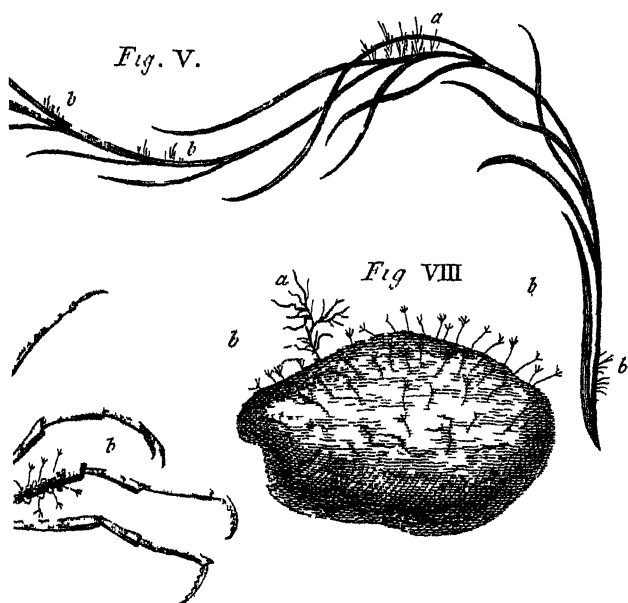
Fig. VI.



Fig. VII.



I. Rhodiūſ ad verum pinxit.



exhibet, erant millia, celerrimo motu vel repentia vel natantia: sex suis pedibus postremis ramulum arripiebant, ut erucarum, quas geometras appellant, mos est, et mirum in modum prone et supine se flectentes, de ramo in ramum saliebant quasi agilissimi. Inter hæc erant pauca cæteris, majora, quæ ^f naturali et ^g aucta magnitudine delineare curavi. ^h Alterum animal non minus mirum erat: sed horum omnium cognitio multo melius delineationis inspectu, quam ex valde prolixa descriptione peti potest.

Sed si omnia insecta marina, quæ in diversis corallinis reperi, delineare vellem, infinitum opus susciperem; nam eorum numerus et diversitas captum nostrum superant.

Hæc ergo, ut spero, sufficient ad demonstrandum corallinas non polyporum opus aut fabricam esse, sed his et plurimis aliis insectis marinis domicilio et perfugio aut alimento inservire.

Dabam Zirizocæ in Zelandia,
17 Martii 1757.

TABULARUM EXPLICATIO.

TABULA SEPTIMA.

Fig. I. Exhibet corallinæ plantam, quæ corallina muscosa, sive muscus marinus tenui capillo sperophorus vocatur.

Fig. II. Corallina ramulis dichotomis teneris capillaribus rubentibus.

Tab. X. fig. II. B.

^g Ibid. C.
N n 2

^h Ibid. fig. III.
Fig.

Fig. III. Junior planta corallinæ tubulariæ laryngi similis.

Fig. IV. Duæ species *a*, *b*, *fig. I.* et *II.* et *c* eschara papyracea utrinque cellulifera, uni basi adnatæ, quod sæpius in doliis marinis coniformibus contigit.

Fig. V. Corallinæ rubræ ramulus, quem per aliquot hebdomadas in aqua marina sæpius renovata servavi, quo tempore parvi ramuli *a*, *a*, multum creverunt, et alii *b*, *b*, pullulaverunt.

Fig. VI. Pars conchæ ostrei, in qua, præter filamenta quædam viridia, duo polypi *a a* conspiciendi.

Fig. VII. Cancer arachnoideus, cui duæ polyporum species infidebant. Singulus in *a*, et multi cellulas habitantes in *b*.

Fig. VIII. Animal, quod *aarfgat* vocatur, et januis emissariorum veterioribus et navibus accrescit: huic parva corallinæ planta erat innata, in qua nullos detegere poteram polypos; plurimos vero *b. b.* ipsi animali infidentes.

N. B. Caudas horum et præcedentis figuræ polyporum nimis longas delineavit pictor, ut eo melius in conspectum venirent.

TABULA OCTAVA.

Fig. I. Ramus corallinæ rubentis magnitudine naturali.

Fig. II. Idem microscopio visus, et tres polyporum species in eo conspiciendæ.

a. b. Duæ diversæ species caudâ vel corporis parte posteriore corallinæ affixæ.

c. Tertia species in cellulis habitans.

d. Poly-

Fig. I.



Fig. II.



Fig. III.



Fig. IV.



Fig. V.



Fig. VI.



Fig. VII.



d. Polypus mortuus.

e. Polyporum cellulæ.

Fig. III. Planta corallinæ tubulariæ laryngi fimilis magnitudine naturali.

Fig. IV. Hujus plantæ ramus maximus, microscopio visus, in quo quinque diversos polypos inveni.

a. Prima et maxima species polypi, quem coccineum voco, et tab. IX. fig. II. fortiore adhuc microscopio visum exhibet.

b. Eadem sed minor species.

c. Tertia, quæ eadem ut hujus tabulæ fig. II. litt. *b.*

d. Quarta, quæ eadem ut hujus tabulæ fig. II. litt. *c.*

e. Quinta et minima polyporum species, maxime aucta magnitudine adhuc delineata fig. I. tab. IX.

f. Cellulæ, quas quarta species habitat.

Fig. V. Corallina erecta pennata denticulis alternis caule appressis: in hac nulli erant polypi nisi in cellulis circumcirca truncam affixis *a a.*

b. Cochleæ magnitudine auctæ in B.

c. Eschara millepora minima crustacea cellulis tubiformibus, animalculis domicilio inserviens, et magnitudinæ auctæ in C.

Fig. VI. Corallina abietis forma, quam mense Decembri accepi: ejus rami vesiculis vel ovulis *a, a,* per paria ordine quadam positis, erant obsessi.

A. Talis vesicula vel ovum microscopio visum.

b. Cochleæ, & *c.* Eschara minima, ut in præcedente figura magnitudine aucta in B et C.

d. d. Dua corpuscula fusca, quæ microscopio visa nidum vermium referunt in D.

Fig. VII. Corallina pennata et filiquata, ab ostreo abstracta: in hac præter tres polyporum species

a A,

a A, *b* B. (quæ cædem ac in fig. II.) *c* C, sex alia infecta reperire contigit, quæ delineata sunt in tab. X. fig. 1, 6, 8.

TABULA NONA.

Fig. I. Minimorum polyporum marinorum genus, cum polypis ramosis (*polypes à bouquet*) aquæ dulcis conveniens.

A. talis polypus conservæ marinæ viridi infidens vix oculo nudo conspicuus.

B. idem lente oculari, et in C fortiore visus microscopio.

Fig. II. Polypus coccineus, quem tabulæ secundæ

Fig. III. & IV. naturali et aucta magnitudine exhibent, hic fortissimo microscopio visus.

A. hic polypus expansis brachiis, prædam expectans.

B. idem brachia contrahendo, prædam arripiens.

a. Brachia majora inferiora numero 16, 18, vel 20.

b. Brachia superiora breviora numero 12, 14, vel 16.

c. Corporis pars superior pyriformis, inferiori infixa.

d. Corporis pars inferior compressa.

e. Locus, ubi polypus corallinæ inhæret.

C. idem polypus a parte anteriore visus, cum corporis partem superiorem pyriformem in globulum contraxerat, quod in majoribus polypis (vide infra fig. IV, V, VI.) magis visibile.

Fig. III. Similis polypus coccineus, cæteris major, ex cujus corpore (ubi partes *c* et *d* conjunguntur) octo ramuli enascebantur, qui in summitatibus duos vel tres gerebant globulos, punctum rubrum in medio habentes, et quos in polypos juniores excreturos fore speraveram frustra.

a. Hujus

Fig 1

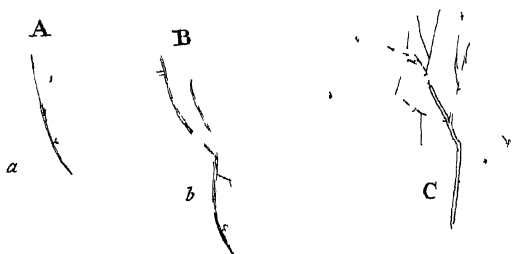


Fig III.

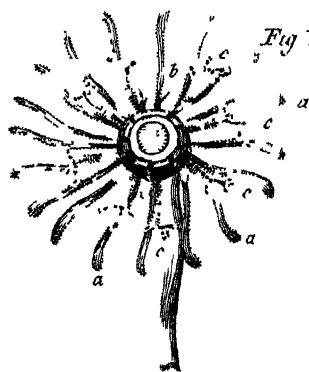
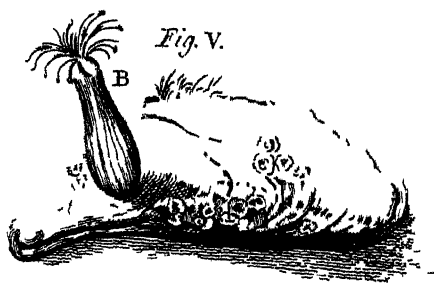
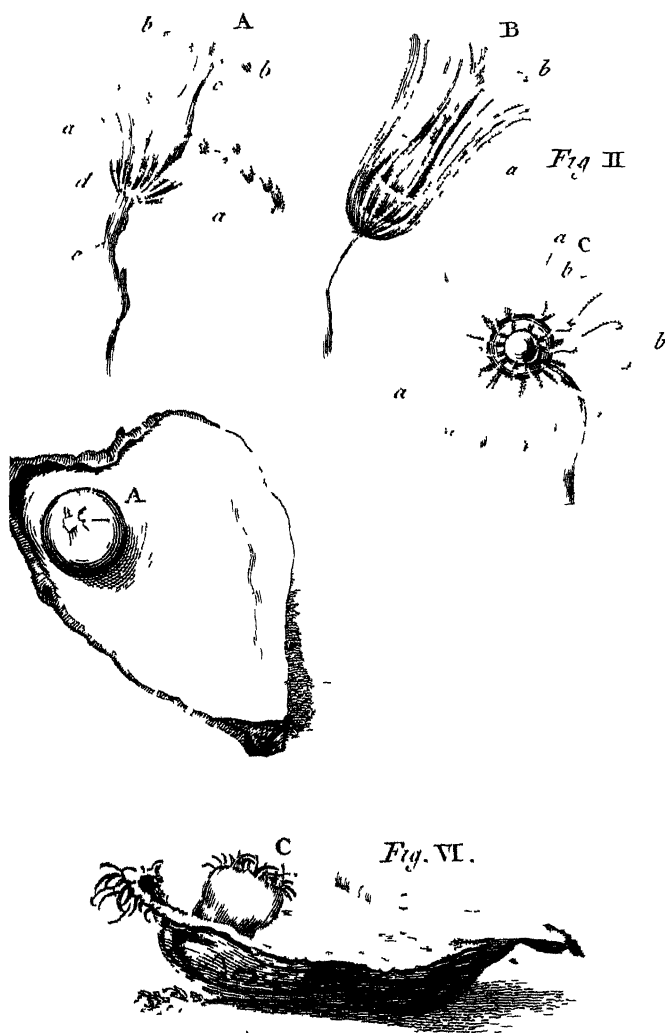


Fig IV.

Fig. V.



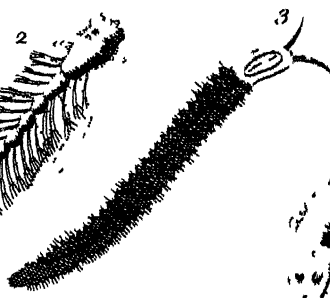
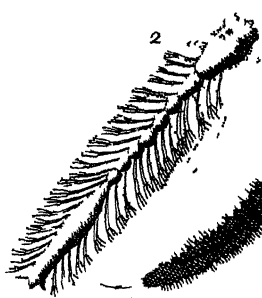
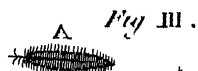
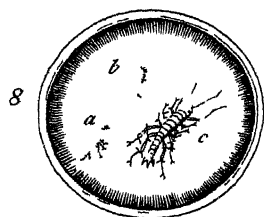
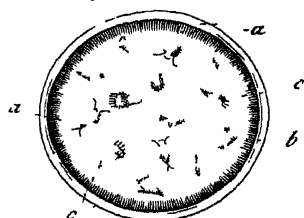
I Rhodius ad vivum pinxit.



A

B

Fig. I.



I Rhodius ad vivum pinxit.



a. Hujus polypi brachia longiora inferiora.

b. Brachia breviora superiora.

e. In medio corporis pyriformis os polypi esse videtur.

Fig. IV. Majus, ut videtur, genus polyporum, quos *klapkanten* vocant, ostreorum conchis insidentium, quorum hic, rudius attractum, brachia penitus in corpus suum abscondit.

Fig. V. Idem polypus corpore extenso brachia expandens.

Fig. VI. Idem capta præda se contrahens.

TABULA DECIMA.

Fig. I. Tres species animalculorum lucentium in guttula aquæ marinæ fortiore microscopio visorum.

Fig. II. Mirum animalculum in corallinis a doliis marinis coniformibus abrafis repertum.

A. tales minores erant centeni.

B. decem vel duodecim erant hac magnitudine naturali.

C. idem animal microscopio visum.

a. Antennæ.

b. Primum par pedum vel brachiorum.

c. Secundum par.

d. Tertium et maximum par.

e, e, e, e. Quatuor corpuscula oviformia, quæ animal ~~ne~~ movebant natando.

f, f, f, f, f, f. Sex pedes posteriores, quibus simul corallinæ ramum arripiens, quaquaversum se flectere poterat.

g. Cauda in cujus extrema parte anus.

h. Oculi.

Fig.

Fig. III. Aliud animal in iisdem corallinis repertum.

A. illud animal pronum.

B. supinum.

C. fortiore microscopio visum.

Fig. 1, 2, 3, 4, 5, 6, 7, 8. exhibent quasdam noctilucas, et alia animalcula in diversis corallinis reperta, ea magnitudine, qua tertia et quarta lens microscopii à Do. Cuff in Anglia fabrefacti illa ostendit. Animalculum c, fig. 8. mirabilissimæ erant structuræ, et plurima habebat membra.

XXXIII. *Remarks on Dr. Job Baster's Observations de Corallinis, &c. printed above, p. 258. In a Letter to the Right Honourable George Earl of Macclesfield, President of the R. S. from Mr. John Ellis, F. R. S.*

My Lord,

Read June 9. 1757. **I** HAVE read Dr. Job Baster's letter to the Royal Society; wherein he endeavours to prove, that corallines are not of an animal, but a vegetable nature; and has brought many arguments to support his system; which, to gentlemen not well acquainted with the subject, may appear plausible.

I could have wished the Doctor had read and examined thoroughly what has been lately written on the subject: I then should not have had occasion
to

to trouble your Lordship with the following remarks, which I find necessary to support what I have already advanced on that head.

His first argument is, That because he does not find as many polypes in the corallines adhering to ships, flood-gates, and buoys, as in deep water on oysters, muscles, and rocks, therefore he concludes, that corallines are not formed by polypes.

In answer to this, let us examine the pliable structure of these bodies, and how wisely nature has defended such tender substances with a tough thin membranaceous covering, and we shall find, that the sea is calm enough often near the surface to give them time to grow, even in the strongest currents: but, without doubt, they are more liable to be destroyed in such agitated situations, than in the calm depths of the sea.

His second argument is, That finding polypes are not equally dispersed over the whole plant, how can they form it? and gives us an example, *Tab. VIII. fig. 5.* of a coralline, that is incrusted with many other corallines or polypes on the stem, but has none on the branches.

Here we plainly see the mistake: the Doctor looks for the tender part of the polype on the surface of the coralline, considering it as a plant; and indeed, if this was the case, he ought so to do; but he never once takes notice of the internal hollow structure of the stem, branches, and denticles of those bodies, to inform us, whether he found an animal in those parts or no. This material point he seems not to have thought on; which is really the true point in controversy at present among gentlemen, who have not examined these bodies recent in sea-water.

His third argument is, That almost always one and the same coralline plant cherishes polypes of different kinds; and refers us to Tab. VIII. fig. 2. and 4.

In fig. 2. he gives us an elegant painting of a geniculated red conferva for a coralline, furrounded, as is very common, by many species of small corallines and escharas. And in fig. 4. he gives us a drawing of one of the tubular corallines, with the head of the animal at the top of it; the stem of this is incrusted with four different corallines and escharas, like the conferva fig. 2; and then he asks, which of these five polypes made the tubular coralline?

To give him some proof of the animal nature of this coralline, let him consult Ray's Synopsis, ed. 3. p. 34. n. 4. and there he will find one of this species, called *adianti aurei minimi facie planta marina*, taken notice of so long ago as the year 1713. by Dr. Lloyd, as a zoophyte, from its stem or tube's being full of a thick reddish liquor, rather resembling blood than the juice of a plant; which, upon pressing the stem, communicated with the little head at top.

His fourth argument is, That as upon one and the same coralline plant you shall find different kind of polypes; so, in different species of coralline, the same polypes: and, to confirm this, he quotes my Essay on Corallines; where I have remarked, that the polypes in the denticles of the setaceous or bristly coralline, N^o. 16. appear to be like those, that are on the lobster's-horn coralline, N^o. 19. And to illustrate this, he observes, that bees and wasps always build their cells invariably the same; and that therefore these two corallines should be the same.

But

But herein he takes this matter wrong: he has considered, in all his observations, the heads of those parts of the polype, in which are the mouths, arms, or tentacula, which appear coming out of the cups, denticles, and at the ends of the tubes of the corallines, as so many whole and intire animals, without ever observing, that the body of the animal is contained in the tubular part of the root, stem, and branches; and that these differ from one another widely both in size and shape, as he may plainly see in the two corallines he has instanced: for the more exact drawings of which, I shall refer him, *viz.* for the setaceous or bristly coralline, to my Plate, N^o. 38. the natural size of which is at fig. 4. and the magnified one at fig. D: this he will observe to have a small stem, and its branches disposed in a pinnated form: and for the lobster's-horn coralline, I shall refer him to Tab. xxii. of Vol. xlviii. of the Philosophical Transactions; where, at N^o. 3. the natural size is expressed, and at C the upper part of this coralline is drawn in proportion to the bristly coralline from the same magnifying glass; which shews the stem to be much larger, and surrounded by its branches growing in whorles at equal distances, not unlike the equisetum, or horse-tail plant; and yet the heads of this animal nearly resemble the other, only a little larger. Further, his comparison to bees and wasps, and their cells, is not conclusive: for these ramified, hollow, and denticulated bodies, called corallines, which we so frequently find dead on our shores, are properly skins of certain marine polypes, and not nests, as those constructed by these little winged animals are. And yet we find as great a

regularity in the same species of these corallines, as when we compare two oak trees to one another, or two of Mr. Trembley's branched fresh-water polypes to one another.

He then proceeds to his fifth argument, That if corallines were formed by polypes, neither the polypes, nor even their cells, would ever fix on living animals, or any other bodies.

Here we may observe, that the consequence he draws doth not follow: for corallines may be formed or produced by certain species of polypes, and yet polypes of another species may be found adhering to other bodies, and even to animal bodies.

By his sixth argument he endeavours to prove, That the vesicles, which are found in regular rows on the sea-fir coralline in winter, Tab. VIII. fig. 6. do not belong to it; and are no more than the eggs of some sea insect deposited on it, of which there may be a great variety.

But to convince him of his mistake, let him take off one of the vesicles, and apply a large magnifier to the place, and he will discover a hole, by which this vesicle or ovary has had a communication thro' the skin with the parent polype. For a further illustration of the manner in which these vesiculated polypes breed, let him consult the 38th Plate of my Essay, where he will find several accurate figures (drawn by Mr. Ehret from the life) of these vesicles, with the spawn of the polypes coming out of them; some of which spawn we evidently discovered to be young polypes with their arms formed; and, as they fell from the vesicle, extending themselves in the watch-glass of sea-water.

In examining the drawings for his plates, I have observed, that Tab. VII. fig. 2. is evidently a red conferva, which he calls a coralline. We have no corallines, but many confervas, of this form and bright red colour on our coasts; and these shores, I believe, are allowed to have similar marine productions with those of Holland.

Tab. VII. fig. 5. he calls a branch of red coralline, which he says he kept several weeks in sea-water, and that often changed; during which time it sprouted and grew very much. This experiment, I am persuaded, is very true; because it is plainly a vegetable, as appears from his own exact drawing of it; and seems to be the *fucus teres rubens minus in longum protensus* of Ray's Synopsis, ed. 3. p. 51. N. 53. This is one of his principal arguments to prove the vegetation of corallines.

Tab. VIII. fig. 1. he calls a branch of red coralline; and at fig. 2. he has it magnified, where it appears to be a geniculated red conferva, drawn and painted with great exactness.

These arguments, my Lord, and these figures of real vegetables, which the Doctor has given us for corallines, shew, how much he is willing to support the old opinion of the botanists: but I am satisfied he will soon alter his opinion, when he observes the remarkable difference of the texture of vegetable and coralline bodies, when viewed in sea-water thro' a good aquatic microscope. And to convince him more fully, that corallines are an animal substance, let him burn them, and he will perceive the same pungent volatile alkaline smell, which he finds in
burning

burning horn, hair, or oysters ; whereas burnt fucus's and confervas yield a smell not much unlike that of common land vegetables. Even the stony corallines, when their cretaceous covering has been dissolved in vinegar, the membranous part, that remains of them, put into the fire, yields the same animal smell with other corallines.

Further, since I find the Doctor has promised the Royal Society to continue his researches at the seaside, the following hints may be of use to him. And, first, he will find, that those he seems to think naked polypes, which he found adhering to corallines and other bodies, are really small corallines and escharas, with their proper skins and cells ; all which I have particularly described already. I would then recommend him to examine such corallines as are taken out of the deepest water, which are found adhering to shells and fucus's. He will find Mr. Cuff's aquatic microscope, or one of that form, the most commodious for observing these animals alive.

The most transparent ones he will find the best to discover their gelatinous inside, which runs thro' the stem and ramifications, and ends in the heads, where the claws are. Some of the best kinds to observe are as follows : The sea-oak coralline, the lily-flowering coralline, the great tooth coralline, the sea-thread coralline, and the branched tubular coralline. Pieces of these should be cut off while they are in the sea water, and placed in watch-glasses full of the same : in these they should remain a while, till they recover themselves ; and when they are placed on the stage of the microscope, the motion of the internal part communicating with the heads will be easily discovered.

If the Doctor will immerse some of these coral-lines, when they are extended, in two thirds of spirit of wine and one third of clear sea-water, it will preserve them many years, as I have experienced. He may then put the different sorts into distinct phials, and view them at pleasure with a lens of about one inch and half focus.

In fine, my Lord, opportunities so seldom offer at the sea-side to make these experiments with accuracy; add likewise to this, the strong lines of vegetation that these bodies carry in their appearance, and your Lordship will not be surpris'd, that there are so many gentlemen, even of the Royal Society, that totally disbelieve them to be animals.

Many there are in the Society, that are wavering between both opinions. If then, my Lord, you think, that any specimens which I have, or any demonstrations tending to clear up this point, that lie in my power, will be acceptable to your Lordship and the Society, your Lordship may freely command them, whenever you think proper, from

Your LORDSHIP'S

Much obliged and most obedient Servant,

London,
June 9. 1757.

John Ellis.

XXXIV. *An Account of an extraordinary Operation performed in the Dock-Yard at Portsmouth: Drawn up by Mr. John Robertson, F. R. S.*

Read May 26.
1757.

THE Royal William, a first rate man of war, built about 40 years ago, having, upon examination, been judged in so good a state, as to be worthy of repairing for sea service, was ordered into dock, and brought thither on the 29th of June 1756. On these occasions it is usual to lay across the middle line of the bottom of the dock, at distances of about five feet from one another, thick pieces of oak timber of about four feet long; their upper surfaces lying in the same plane, or so posited, that a line stretched from the two extreme blocks will touch all the intermediate ones; and on the middle of these blocks the keel of the ship is to rest. On the said day the tide did not rise so high as was expected; and there was not quite depth enough of water to float the ship in, and set her on the blocks, notwithstanding the assistance of an empty lighter, which, being fixed to the stern, lifted the ship at that end six inches: and as the officers knew they should not have so much water again before the next spring-tides, they were determined to heave her in; which is a very common operation in most dock-yards. Now it so happened, thro' the great weight of the head and stern, that the ship cambered very much; that is, her keel, from being strait, was become much curved, the

two

two extremities hanging lower than the middle part by many inches ; and consequently the foremost part of the keel, instead of sliding over the blocks, forced all the foremost ones away, for above 60 feet ; whereby that part of the keel rested on the bottom or floor of the dock, and the aftermost part rested on such of the blocks, as had escaped the violence, which had displaced the others. In this situation the keel was very far from being strait ; and so it was resolved to lift by main force the head of the ship, until the keel should be strait ; and in that position to support it by the blocks, which had been forced away from their places.

For this purpose there were set up, under the wales and other parts of the ship, to the length of near 80 feet of the stem, as many shoars, as were judged necessary ; and also nine pair of bed-screws, three pair under each bow, and three pair under the knee of the head. At each shoar a workman was appointed, to drive wedges between the heels of the shoars and the parts of the dock whereon they rested ; whereby the shoars were raised end-wise, and consequently the body of the ship lifted at the same time. While this was doing, the 18 screws were also at work : and between these efforts the fore part of the ship was raised upwards of 19 inches, so much being necessary to bring the fore part of the keel in a right line with the hinder part.

In this service were employed about 270 men ; whereof about 144 worked at the screws, and the others worked at the shoars with their mawls and wedges ; and the whole operation was performed in about seven hours.

My curiosity leading me to inquire what was the weight of the ship, in the condition she was at the time of bringing her into the dock; for this purpose I procured draughts of the elevation and section, and of the plans at the line of floating, and at the parallel sections of every foot distance down to the keel. Then, by finding the mean area between every two sections, I was thereby enabled to come at the magnitude of a solid, that would nearly fill the trough the ship made in the water; and, by increasing this magnitude by that of the keel, and so much of the stern-post and stem, as were under water; the cubic feet of the fluid displaced by the ship were obtained; being 54869; and consequently her weight was 3532091 pounds, or 1576 tons, 16 C. 2 qrs. 3 lb. These numbers were not altogether so easily come at, as they would have been, had the ship swam on an even keel, her draught of water before being 13 feet 2 inches, and abaft 16 feet 6 inches. However, the computation may be esteemed as correct as the nature of the subject would admit; because I found pretty near the same solidity by another method.

I got a block or model made, by a scale of a quarter of an inch to a foot, of so much of the Royal William's body, as was immersed, when she was brought into dock; and this block I immersed in a trough of sea-water, and found its weight in the following manner.

The length of the trough was 46 inches, breadth 14 inches, and depth 8 inches: at each corner was a graduated scale of inches, and pencil-lines drawn round the inside of the trough at every inch. Sea-water was poured into the trough to the height of 5 inches;

inches; and the trough was exactly levelled, by means of the pencil-line, at .5 inches: then the block being forced under the water's surface, the fluid, when still, was risen to $6\frac{1}{2}$ inches; consequently the magnitude of the block was equal to a parallelopipedon of 46 inches long, 14 inches broad, and $1\frac{1}{2}$ inches deep, or to $858\frac{2}{3}$ cubic inches.

Now $858\frac{2}{3}$ cubic inches are equal to 0.4969 cubic feet.

And a cubic foot of sea-water weighs 64.3732 pounds avoirdupoise.

Then $64.3732 \times 0.4969 = 31.987$ pounds.

So that by a quarter inch scale, a model similar to the Royal William weighs near 32 lb.

But a quarter inch scale is $\frac{1}{48}$ of a foot scale.

And the model is to the ship as 1^3 is to 48^3 , or as 1 is to 110592.

Then 3537506 lb ($= 110592 \times 31.987$), or 1579 tons, 4 C. 3 qrs. 14 lb. is the weight sought.

The difference, by the two methods amounts to 5415 lb. or to 2 tons, 8 C. 1 qr. 11 lb.

Some of the persons present at this experiment read the height of the water at $6\frac{3}{8}$ inches: the difference between $6\frac{3}{8}$ and $6\frac{1}{2}$ inches is $\frac{1}{4}$ of an inch; a difference easily to be made by different persons in an experiment of this kind. But observing, that the computation made on $6\frac{3}{8}$ inches amounted to near 50 tons more than on $6\frac{1}{2}$ inches, I caused the trough to be diminished in its depth to $6\frac{1}{2}$ inches, had one of the ends cut off, and a board fixed on the open side, being desirous of making the experiment with the trough standing on one end: and indeed, in this situation, an error of $\frac{1}{16}$ of an inch, in the height

height of the water makes a difference of about $16\frac{1}{2}$ tons in the weight of the ship. Into this upright trough water was poured to the height of 36 inches; and the block being immersed, the water was raised $9\frac{1}{2}$ inches: so that the block was equal in magnitude to a parallelopipedon of 14 inches long, $6\frac{1}{2}$ inches wide, and $9\frac{1}{2}$ inches deep, or to $849\frac{1}{2}$ cubic inches: from whence I find the weight of the ship to be 1562 tons, 1 C. 2 qrs. 16 lb. And altho' I take this number to be nearest the truth, yet it may be observed, that it is no easy matter to come at accuracy in this subject by any of the methods in common use.

My next inquiry was, to find how much of this weight was lifted, and how to proportion it among the screws and mawl-men: but in this, less accuracy must be expected than in the preceding inquiry; for the exact number of men employed is not known; neither can it be told, how many worked at the screws, and how many with the mawls; and only a guess can be made at the part lifted. However, something may be gathered, which may, perhaps, be worth the knowing.

Let the weight raised be taken at half the weight of the ship; for 64 feet, the length of the keel raised, is not far from half the whole length: add to this the fall of the head, the weight of the forecastle, the friction of the timber, and the resistance of the parts bent by the cambering: beside, the mawls worked at several shoars set up abaft the said 64 feet.

Now the weight by the last experiment was 3499064 pounds: one half, or 1749532 lb. I take
to

to be the weight raised between the screws and mawls.

The distance between two contiguous threads of each screw was $1\frac{1}{3}$ inches; the length of the two opposite levers was 12 feet 8 inches, or 152 inches, and described a circumference of $477\frac{1}{2}$ inches: each screw was worked by 8 men: their force, reckoned at 30 lb. each, makes the power working on each screw equal to 240 lb.

Hence, from the known property, each screw could raise 65485 lb.

And the 18 screws raised 1178730 lb.

Then there remained 570802 lb. to be raised among about 126 mawls:


Which gives 4530 lb., or a little more than two tons, to be raised by each man with his mawl and wedges; which is considerably less than what I have seen raised by way of experiment.

XXXV. *Observations on an Evening, or rather Nocturnal, Solar Iris. By Mr. George Edwards, Librarian of the College of Physicians.*

To the Reverend Dr. Birch.

S I R,

Read June 16,
1757.

 N Sunday evening the 5th of June 1757, being walking in the fields near Islington, about half a mile north of the upper reservoir or basin of the New River, I observed the
fun

sun to sink beneath the visible horizon to the north-west, it being very clear in that quarter, except some thin clouds a little above the horizon, which were painted of fine red and golden colours, as is usual, when the sun sets in a calm clear evening. But, about 20 minutes after sun-set, as near as I could judge, it then being darkish, I was greatly surprised to see an Iris in the dusky air, at a height greater than is seen at any time in the rainbow. It was in the contrary quarter of the heavens to the setting sun, and fell on the smoke, mists, and evening vapours arising from the city of London and its neighbourhood. The arch seemed to be a full half circle, tho' its lower parts fell some degrees short of the horizon. It was very distinctly seen for about 15 minutes. Its colours the same as in the rainbow, but fainter. The lower ends of the bow arose gradually higher from the earth, as the sun declined beneath the horizon, until the whole arch disappeared. The center of the arch was above the horizon at its first appearance. What most perplexed me, was, to find the cause of this painted arch. I could not believe, that it proceeded from the sun-beams falling on rain; for there had been none that afternoon; nor was there any sort of signs of rain or rainy clouds to be seen; the wind being northerly, and the air cool, and somewhat hazy in the quarter where the bow appeared; which was not near so bright as the rainbow appears to be in the day-time; and I believe, that it would not have been visible at all in the presence of the sun. I imagine it was formed on the gross particles of the evening vapours, mixed with those of the smoke arising from the town;

town ; for had the sun-beams shot from beneath the horizon on falling rain at a considerable height above the earth, I believe the darkness would have rendered the appearance of such a bow far brighter than it appears to the sight in the presence of the sun : but this night or evening arch being reflected, as I suppose, from particles so minute as those of floating vapours, gave but little light and colour to the sight, and what would not have been visible, had the sun been above the horizon. For the same reason, the moon and stars are visible in the absence of the sun, and, on the contrary, are unseen when the sun is present : and if we light a candle, and set it in the sun-beams, the flame is lost to our sight, tho' the same candle will give us a considerable share of light in the night. As I have never before seen or heard of such an arch, I thought this account of it (imperfect as it is) might not be disagreeable to the Royal Society.

It could not be a lunar arch, the moon being then many degrees below the horizon, and the arch in a place, where it could not be affected by the moon's rays. The consciousness of my inability to give a proper account of such an uncommon appearance could not deter me from the attempt.

I think I have said all that is necessary on this subject ; yet am ready to answer any question for the farther illustrating of it.

Reverend Sir,

Your most humble servant,

College of Physicians, London,
don, June 6th 1774.

Geo. Edwards.

XXXVI. *The Effects of the Opuntia, or Prickly Pear, and of the Indigo Plant, in colouring the Juices of living Animals.*
Communicated by H. Baker, F. R. S.

June 23^d, 1757.

Read June 23, 1757. **M**R. Baker received a letter yesterday from Dr. Alexander Garden, of Charles Town in South Carolina, part of which he hopes he shall be excused for laying before the Royal Society.

The Doctor writes thus : — “ As you desired, I
 “ tried the effects of the prickly pear in colouring
 “ the urine. A few days after your letter, I went
 “ down to one of the islands, and gathered some of
 “ the fruit, and gave four of the pears to a child of
 “ three years of age, and six pears to one of five.
 “ The next morning I examined the urine of both,
 “ and it appeared of a very lively red colour, as if
 “ tent-wine had been mixed with clear water. The
 “ urine of the eldest was deeper coloured, and of a
 “ darker look : the youngest (who always naturally
 “ made clear urine) was of a more lively and beau-
 “ tiful red. Next day I gave six pears to a Negroe
 “ wench, who gave suck, and strictly forbade her
 “ suckling her child for six or eight hours ; and
 “ then taking some of her milk in a tea-cup, and
 “ setting it by for some hours, the cream had a red-
 “ dish lustre, tho’ it was very faint.

“ I was led to this last experiment by an observa-
 “ tion, which I made on the milk of cows, who
 “ had

“ had fed in an indigo-field: the indigo had not
 “ only tinged their urine blue, but the cream of
 “ the milk was of a most beautiful blue colour, and
 “ had a radiated appearance from the centre (Is it
 “ not hence probable, that the dye is the oily part
 “ of the plant?). The milk underneath was clear
 “ and white as usual.”

Dr. Garden wrote, a year ago, that the prickly pear grows in great abundance about Carolina; and also that the cochineal insects are found upon it; but hitherto no attempts have been made to cure them as the Spaniards do. In hope, that some rich dye may be produced from the plant itself, Mr. Baker proposed some experiments to Dr. Garden, which he intends to prosecute this summer.

XXXVII. *An Account of an extraordinary Shower of black Dust, that fell in the Island of Zetland 20th October 1755*. In a Letter from Sir Andrew Mitchell, of Westshore, Bart. to John Pringle, M. D. F. R. S.*

S I R,

Pall-Mall, June 9th, 1757.

Read June 23, 1757. **I**N compliance with your desire, I made particular inquiry, whether at or about the time the earthquake happened at Lisbon the 1st of November 1755. any uncommon phænomena

* See Phil. Transact. Vol. xlix. Part 2. p. 509.

were observed to appear in the islands of Orkney or Zetland, as such had happened about that time in other parts of Scotland. From Orkney I was informed, that nothing particular had happened; only, that about the time mentioned the tides were observed to be much higher than ordinary. I received from Zetland a letter, dated 28th May 1756. from Mr. William Brown, Master of the grammar-school at Scalloway in [that country, a sensible and observing man; wherein he writes verbatim as follows. “ Blessed be
 “ God, notwithstanding the great devastations, that
 “ have been made in other parts of the world by
 “ earthquakes, we have been intirely free from any
 “ disaster of that nature: nor has any thing extra-
 “ ordinary happened in this country since you left
 “ it; only on Monday the 20th October last, be-
 “ twixt the hours of three and four in the afternoon,
 “ the sky being very hazy, as it uses to be before a
 “ storm of thunder and lightning, there fell a black
 “ dust over all the country, tho’ in greater quantities
 “ in some places than in others. It was very much
 “ like lampblack; but smelled strongly of sulphur.
 “ People in the fields had their faces, hands, and
 “ linen, blackened by it. It was followed by rain.
 “ — Some people assign the cause of it to some ex-
 “ traordinary eruption of Hecla. But I shall trou-
 “ ble you no more about it, as no doubt some of
 “ your friends have written to you of it some time
 “ ago.”——

In June 1756. I returned to Zetland; and, upon further inquiry, found what Mr. Brown had written me was attested by Mr. Mitchell, parson of the parish of Tengwall, and by several Gentlemen of credit and reputation, who had seen and observed
 the

the same phænomenon in different parts of the country at the time above-mentioned.

Mr. Brown having omitted to mention, how the wind did blow at the time the black dust was observed, I made particular inquiry about that circumstance, and found it was from the S. W. which does not seem to favour the opinion, that the dust proceeded from an eruption of mount Hecla, which lies about N. W. from Zetland; unless it may be supposed, that a north wind happening just before had carried this dust to the southward, and the south-west wind immediately following had brought it back to the northward. But, in this case, would not this black dust have been observed in Zetland at its first travelling to the southward? Upon inquiry, I did not hear it was.

Thus far I have obeyed your commands, which I will always do with pleasure; and if you think it worth while to lay this letter before the Royal Society, I leave you at full liberty to do so, or not, as you think proper: but what it contains may be relied on as truth. I am, with great regard,

Dear Sir,

Your most obedient humble Servant,

And. Mitchell.

P. S. I may add, that the distance from mount Hecla to Zetland is between 500 and 600 miles.

XXXVIII. *A Description of some Thermometers for particular Uses.* By the Right Honourable the Lord Charles Cavendish, V. P. R. S.

Read June 30, 1757. **T**HE thermometer (TAB. XI. fig. I.) is designed for shewing the greatest degree of heat, which happens in any place during the absence of the observer. It consists of a cylinder of glass joined to a tube, and differs from common thermometers only in having the top of the stem drawn out into a capillary tube, which enters into a glass ball C, joined on to the stem at the place where it begins to be contracted. The cylinder, and part of the tube, are filled with mercury; the top of which shews the common degrees of heat as usual. The upper part of the tube above the mercury is filled with spirit of wine, and some of the same liquor is left in the ball C, so as to fill it almost up to the top of the capillary tube.

Now when the thermometer rises, the spirit of wine will be driven out of the tube, and will fall into the ball C. When the thermometer sinks again, as the spirit cannot return back from the ball, the top of the tube will remain empty, and the length of the empty part will be proportional to the fall of the thermometer. Therefore, by means of a proper scale, the top of the spirit of wine will shew how many degrees it has been higher than when observed; which being added to the present height, will give the greatest degree of heat it has been at.

To,

C *Fig. 1.*

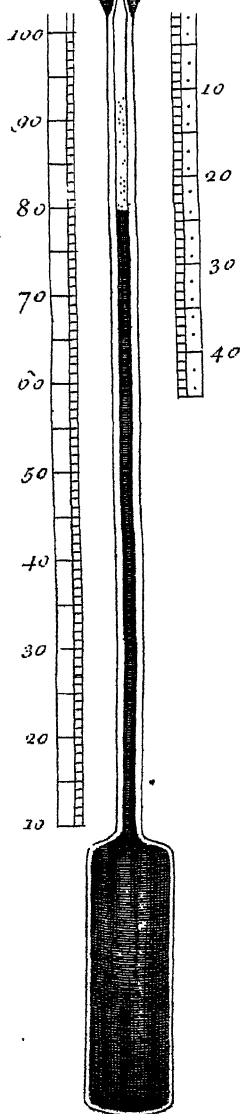
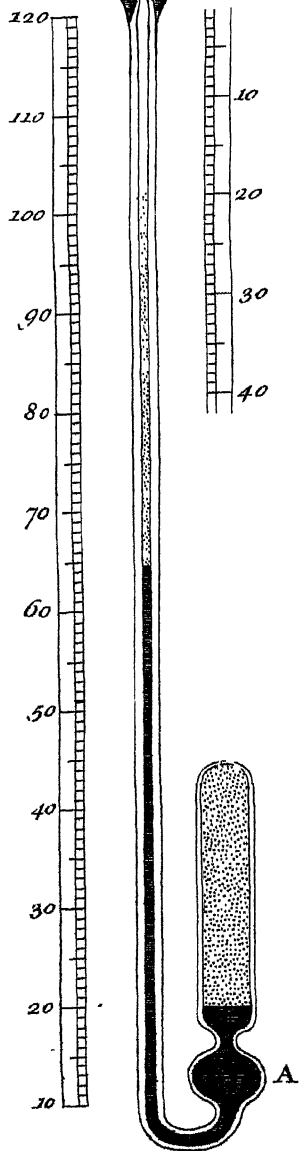
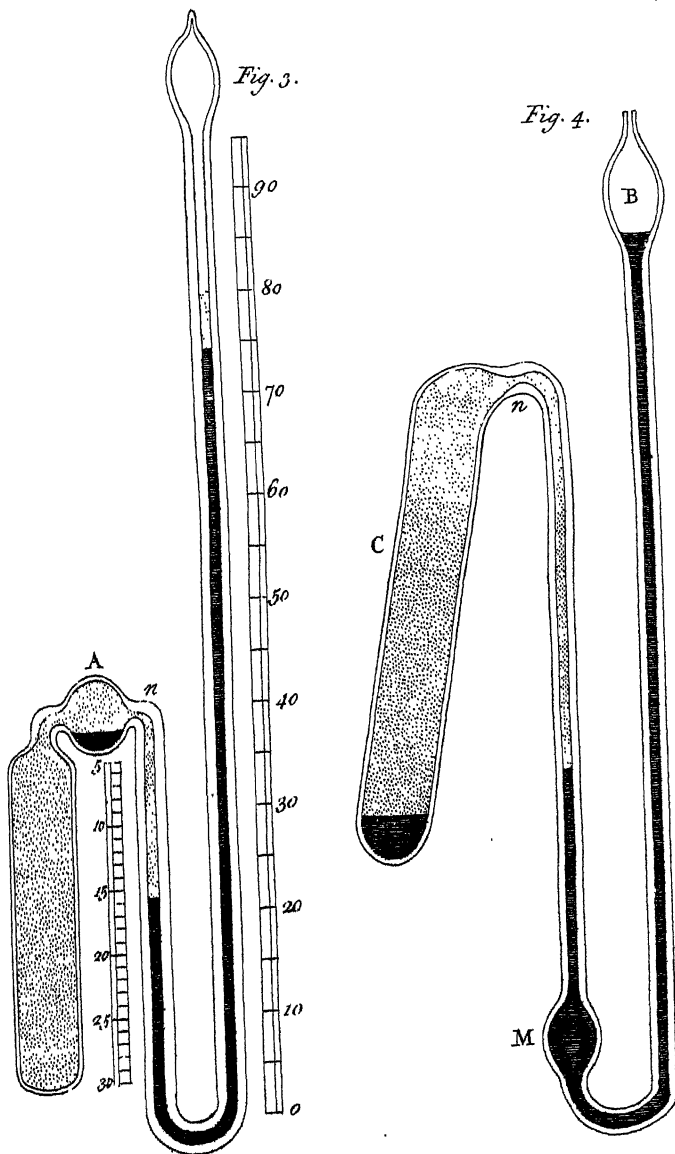


Fig. 2.





To fit this thermometer for a new observation, it is necessary to fill the upper part of the tube with spirits ; which may be done, by inclining the instrument till the spirits in the ball C cover the end of the capillary tube. For if the cylinder is then heated, by applying the hand to it, or by the flame of a lamp held at some distance, till the spirits rise to the top of the tube and run over into the ball C, and is then suffered to cool in the same position, the tube will remain full of spirits, and the thermometer will be fitted for a new experiment.

The top of the capillary tube is made to stand pretty near to one side of the ball, and also to the top of it, that a less inclination of the instrument may be sufficient to make the spirit of wine in the ball cover the end of the tube.

The ball C is joined on as high as possible, so as to hide no part of the tube, except that, where the bore is contracted. By this means, the top of the spirit of wine begins to appear before the thermometer has sunk one degree.

It will be convenient to leave some mercury in the ball C, which may be made to cover the end of the capillary tube, by inclining the thermometer more than what is necessary to make the spirit of wine cover it. By this means some mercury may be got back into the tube, in case any of it should happen to be driven into the ball by the thermometer's being exposed to too great a heat.

The scale of degrees at top, which shews the descent of the thermometer from the highest point it has arrived at, ought not, in strictness, to be the same at all times of the year ; for those degrees exceed
the

the common degrees of heat pointed out by the top of the mercury, as much as the column of spirit of wine expands, and therefore are greatest when that column is so; that is to say, when the greatest heat to which the instrument has been exposed is least. A difference of 30 degrees of Fahrenheit's scale, in the greatest rise of the thermometer, would require the scale to be altered one sixtieth part: and the error arising from making use of the same scale will be about one sixth of a degree, if the thermometer is observed when it has fallen ten degrees.

In the instrument here described, the bore of the tube is about 0.027 inches; and one inch of it contains two grains of mercury, and answers to about ten degrees, the cylinder containing about 2280 grains. If a much shorter tube was made use of, a considerable error might arise from too great a quantity of spirits adhering to the sides of the tube, in that part, which is filled with mercury; especially when the thermometer rises fast. This makes it necessary to employ a cylinder of a considerable bigness, if it is desired to have the scale of degrees pretty large.

If the weight of the mercury is thought inconvenient, it may be avoided by the construction described in fig. 2. where the bottom of the tube is bent so as to point upwards, and is joined to a ball A, which communicates with a cylinder placed above it. In all other respects it is the same as the instrument before described.

It is filled with spirit of wine and mercury; the quantity of the latter being sufficient to fill the whole tube and the ball A.

No part of the spirit, with which the cylinder is filled, can get into the tube, as long as the instrument is kept in an erect position, or even if it is carefully laid down flat on a table. For tho' in this last case some of the spirits may get into the ball A, it will rise to that part of the ball, which is then uppermost, and will not touch the orifice of the tube *n*; which was the reason for adding this ball, which would be unnecessary, if the instrument was kept constantly erect, or nearly so. If the spirit should come to touch the orifice of the tube *n*, it would work up between the mercury and the glass; which would put the instrument out of order.

The thermometer fig. 3. is designed for shewing the greatest cold, which happens in any place during the time the instrument is left in it. The tube is bent into the shape of a syphon of unequal legs standing parallel to one another, the bend being at the bottom. The top of the shorter leg is bent to a right angle, and immediately opens into a ball A, which, by means of a short bent tube on the opposite side, communicates with a cylinder standing parallel to the legs of the syphon, and pointing downwards. This cylinder contains the greatest part of the fluid; and is added only to make the thermometer more sensible than it would be, if the ball A was made of a sufficient bigness to contain the proper quantity of fluid. This instrument is filled with spirit of wine, with the addition of as much mercury as is sufficient to fill both legs of the syphon, and about a fourth or fifth part of the ball A.

The

The common degrees of heat are shown by the top of the mercury in the longest leg, or by the top of the spirit, in case any of it is left above the mercury.

When the mercury in the longest leg sinks by cold, that in the shorter leg will rise, and will run over into the ball A; from whence it cannot return back when the thermometer rises again, as the surface of the mercury in the ball is below the orifice of the tube *n*. Therefore the upper part of the shorter leg will be filled with a column of spirits of a length proportional to the increase of heat; the bottom of which, by means of a proper scale, will show how much the thermometer has been lower than it then is; which being subtracted from the present height, will give the lowest point that it has been at.

If no further contrivance was used, the mercury would fall into the ball A in large drops; which would make the instrument less accurate. For the thermometer's beginning to rise immediately after a drop is fallen, or just as it is going to fall (in which case it will return back into the tube), will make a difference of such part of a degree nearly as that drop answers to. To prevent this inconvenience, the top of the shorter leg, close to the ball, is contracted, by being held in the flame of a lamp; and the passage is further streightened by a solid thread of glass placed within the tube, and extending from the bottom of the shorter leg to the part near the ball A, where it is most contracted. By this means, as soon as any small portion of mercury is got beyond the end of the thread of glass, it breaks off, and falls into the
ball

ball in very small drops. This thread of glass is fastened by the heat given to the tube in making the bend next to the ball. In order to fill the shorter leg with mercury, to fit the instrument for a new experiment, it must be inclined till the mercury in the ball covers the orifice of the tube *n*. The cylinder being then heated, the mercury will be forced into the shorter leg, and will run down the thread of glass in drops, which will soon unite. By this means, such a quantity of mercury must be got into the shorter leg, as, upon the cooling of the instrument, will be sufficient to drive all the spirit of wine into the ball with a less degree of cold than what the thermometer is likely to be exposed to.

The ball *A* must always have some mercury in it, but never enough to fill it up to the orifice of the tube *n*. It must therefore be made of such a size, as to contain all the mercury, which can come into it from the tube without being too full. If it should happen to be made too small, so as to be too full in cold weather, any part of the mercury may easily be driven into the cylinder, and got back again into the ball when wanted in warmer weather.

It will be better to leave a little of the spirit above the mercury in the longest leg; in which case the top of the spirit will shew the common degrees of heat. For the filling the tube, so as to leave none, is attended with some trouble; and more of it will be apt to get up there, if the instrument should happen to be held in an improper situation, or if it be kept in too warm a place without filling the shorter leg with mercury by the method above described. If too great a quantity should get up, tho' it would

not affect the scale for the common degrees of heat, it would however cause some error in the degrees on the shorter leg; inasmuch as the expansion of that portion of spirits, which has got up into the longer leg, exceeds the expansion of the mercury, which must supply its place. It may be got back at pleasure, by exposing the thermometer to such a degree of cold as will make the spirit get beyond the bend of the syphon; for then it will run up along the thread of glass in the shorter leg till it gets above the mercury there. For this purpose the point of 0 degrees of Fahrenheit's scale should be near the bend; by which means, any part of the spirit of wine may be got beyond it by an artificial cold; and there will be no danger of the whole getting beyond it by any natural cold; in which case the air would get up into the ball.

The scale of degrees on the shorter leg will, in different seasons, be liable to an error of the same kind as that, which was explained in the first-mentioned thermometer; but in this it will be less considerable, as the space between the two scales is filled with mercury, whose expansion is about six times less than that of spirit of wine.

In the thermometer, which I have, the bore of the tube is about 0.054 inches; and one inch of it contains eight grains of mercury, and answers to seven degrees of Fahrenheit's scale. The drops of mercury, which fall into the ball A, answer to about one eighth of a degree.

If instruments of the nature of those above described, were to be used for finding the temper of
the

the sea at great depths, some alteration would be necessary in the construction of them, principally upon account of the great pressure of the water; the ill effect of which can, I believe, be prevented no other way, than by leaving the tube open. For if the thermometer was made strong enough to resist the pressure without breaking, yet it would be impossible to be sure, that the figure of the glass would not thereby be altered, which should make the experiment uncertain.

The instrument for finding the greatest heat might be made just like that of fig. 1. only leaving the top open. It is to be filled with mercury only, as is also the lower part of the ball at top, but not near so high as the end of the capillary tube. The upper part of that ball, being left open, will in a great measure be filled with the sea-water, which will be forced into it by the pressure.

If this instrument (the tube being quite full of mercury) is plunged into any part of the sea, where the heat is greater than that of the air above, part of the mercury will be driven out of the tube; and, upon bringing it into a colder place, the sea-water or air in the ball will enter into the tube, and will fill the space left by the mercury.

As this thermometer does not show the common degrees of heat, it must be placed in a vessel of water with another thermometer, and the scale of degrees at top will shew how much the heat it has been exposed to is greater than that of the water in the vessel.

The sea-water getting into the glass will corrode the mercury, and thereby foul the glass; which

will make the experiment less exact: and therefore it would be much more convenient, if the sea-water could intirely be kept out; which probably may be done by tying a bladder full of air to the neck of the ball C, which will contract by the pressure of the water, without letting any of it get in.

If this can be done, the instrument may be filled with mercury and spirits, just like that at fig. 1. But it would be more convenient to fill it with mercury only: in which case it may be made with as small a tube and ball as common mercurial thermometers: or it may be filled with spirits only. The instrument will thereby become much less bulky; which will compensate the want of the common scale of heat.

It is better to put but little mercury into the ball at top, for fear of its getting into the capillary tube by the motion of the instrument.

The thermometer for finding the greatest cold, if applied to this purpose, must also be left open at top. There is another inconvenience to be avoided; which is, that the mercury in the ball A, by the tossing of the instrument, might sometimes get into the shorter leg of the syphon; which would spoil the experiment. To prevent such an accident, the most convenient construction, which occurs to me, is that of fig. 4. which differs from fig. 3. in having the ball A omitted; so that the mercury running out of the shorter leg will fall to the bottom of the cylinder, and will not be so liable to get back into the tube by motion. The cylinder is made to stand not quite parallel to the legs of the syphon, that the mercury contained therein may more easily be brought to
touch

touch the end of the tube, in order to fit the instrument for a new experiment.

If, by means of a bladder, the sea-water can be kept out of the glass, this instrument may be made to shew the common degrees of heat; but even then, in order to render it less bulky, it may be better to supply the want of them as in the last instrument. The longer leg of the syphon may in that case be made as short as you please; only making the ball B big enough to receive all the mercury, which may be driven into it by heat.

If thermometers of this kind were to be sent up into the air by means of a kite, they might be made like those proposed for the sea; but it would not be necessary to leave them open.

As it would be desirable to make them as small as possible, they should be made so as not to shew the common degrees of heat: and it would also, on that account, be convenient to omit the thread of glass placed within the shorter leg of the syphon in fig. 3. and 4. This thread of glass is placed there in order to make the mercury fall into the ball A. fig. 3. or cylinder C. fig. 4. in smaller drops, and also to facilitate the filling the shorter leg with mercury. The latter purpose may be answered by having a ball blown towards the bottom of the shorter leg, as marked in fig. 4. at M: for as soon as the mercury driven out of the cylinder by heat gets to that ball, it will pass by the spirit of wine. The other purpose may probably be answered by having the tube contracted as much as possible at *n*.

In the foregoing instruments the tubes made use of were of a large bore, as most of the errors in
them

them would increase by making use of smaller ones. Possibly less ones might be used without much inconvenience. The chief advantage will be, the making these thermometers more sensible of the changes of heat, than when large cylinders are used. This will be of service when the greatest degree of heat or cold continues but a short time.

It is better to use plain spirit of wine, than what is tinged, which seems more apt to cause a foulness in the tube, and thereby makes the surface of the mercury less well defined. I am induced to believe so, from observing, that the portion of spirits above the mercury in the tube fig. 3. which at first was strongly tinged with cochineal, in some months became perfectly colourless, the tinging particles being deposited in different parts of the tube, and causing a foulness there. The colour of the spirits in the cylinder does not appear to be altered.

The dark-shaded part in the several figures represents mercury, the dotted part spirit of wine.

Helena. Judith

Judith. Helena



XXXIX. *Observationes Anatomico-Medicæ, de Monstro bicorporeo Virgineo A. 1701. die 26 Oct. in Pannonia, infra Comaromium, in Possessione Szony, quondam Quiritum Bregetione, in lucem edito, atque A. 1723. die 23 Febr. Pofonii in Cœnobio Monialium S. Ursulæ morte functo ibidemque sepulto. Authore Juſto Johanne Torkos, M. D. Soc. Regalis Socio.*

[See TAB. XII.]

Read May 23. 1751. I. **P**ARTUS hic bicorporeus singulare exemplum exhibet admirandarum virium imaginationis maternæ in fœtum utero contentum. Mater enim hujus bicorporis, primis graviditatis suæ menſibus vel potius hebdomadis, attentius contemplabatur canes coeuntes, arctius coherentes, et capitibus erga se invicem quodammodo converſos, eosque sibi crebrius præfigurabat.

II. In partu, primum prodiit umbilicotenus Helenæ corpus; post tres demum horas editi sunt ejus pedes, cum adnexo corpore altero Judithæ. Helenæ corporis statura erat altior et rectior, Judithæ brevior et obliquior; et ~~quædam~~ ^{quædam} infra lumbos, a tergo, in unum corpus concreta fuissent, attamen vultus et corporibus, femilateraliter, erga se fuerant converſæ, ut commode sedere, lentoque gradu procedere et recedere potuerint. Unus communis ipsis erat alvi exitus, intra duas nates, seu Helenæ dextrum et Judithæ sinistrum femur, situatus. Unam quoque habebant vulvam, intra ~~quædam~~ ^{quædam} pedes reconditam, ut ~~data~~

erectis starent corporibus, ne vestigium ejus conspicuum esset. Quoad duos istos excretionum meatus, observatum est, quod, una excretionem alvi moliente, altera quoque nifum egerendi senserit; in reddenda vero urina, quælibet, diverso tempore, stimulos habuerit: quamobrem altera ad urinæ missionem sollicitata, altera subinde recessum negavit. Unde in juventute, utut alias semper semet tenerrime amarent et amplexarentur, sæpius altercationes inter ipsas exortæ, et alterutra aliam vel dorso injectam abripuit, vel colluctando eo, quo vellet, protraxit.

III. Anno ætatis sexto, Judithæ paralyfis totius partis sinistræ; obtigit ex qua affectione, utut convalescisset, per totam vitam suam debilior, tardior, et stupidior perstitit; e contra Helena semper agilior, docilior, et formosior fuit.

IV. Prout diversa erant corpora, ita functionum vitalium, animalium, et naturalium, magna in utroque corpore, tam in sano quam ægroto statu, observata est differentia. Et quamvis variolas et morbillos uno eodemque tempore habuissent, reliqui tamen morbi eis non erant communes. Cum Judith sæpius convelleretur, Helena nec alterata nec debilitata fuit. Helena erat pleuritica. Judith benigniore febre laboravit: alterâ tussi, catarrho, colicâ afflictâ, altera sana exstitit. Hinc etiam quælibet, pro suo diverso statu, diversis medicamentis tractabatur: phlebotomia autem semper in saniore et vegetiore celebrabatur.

V. Anno ætatis decimo sexto, menstrua comparuerunt, quæ deinde per totam vitam, non tamen æquali tempore, modo, et quantitate succedere. Subinde alterutra majores hinc sensit molestias; Judith vero crebrius convellebatur, variisque hystericiis et pectoris affectionibus obnoxia fuit.

VI. Anno ætatis vicesimo secundo, seu A. C. 1723. die 8 Febr. Judith fortiter convulsa est, postea comatosa, usque ad mortem, quæ die 23 Febr. mane contigit, perstitit. Intra hos dies Helena febricula laboravit, eique accesserunt crebriores lipothymiae, quibus tandem ita debilitata est, ut integra quamvis mente et loquela, subito, tribusque horæ minutis prius quam Judith, in agonem inciderit: postea vero ambæ, post brevem agonem, uno ferme momento expiraverint.

VII. Corporibus post mortem dissectis, reperta sunt in quolibet corpore viscera singula: In Helena omnia sana; in Judithæ thorace vero cor nimis magnum, fortissimo pericardio velatum, et pulmonum dexter lobus putridus: Arteria aorta et vena cava ex utroque corde descendentes, antequam arteriæ et venæ iliacæ ex iisdem emergerent, inflexæ coadunabantur, et unam arteriam aortam, unamque venam cavam, e corde uno ad aliud procedentes seu reflexas, præsentarunt. In abdomine utrinque viscera omnia sana et integra. Quodlibet corpus suum habuit hepar, splenem, pancreas, renes, vesicam, uterum cum ovariiis, tubis Fallopiæ, et portione vaginæ, quæ utrinque concurrentes unam communem vaginam efformarunt. Partes genitalium externorum, præter commune orificium vaginæ, cuilibet erant propriæ, velut clitoris, nymphæ, orificium urethræ; alæ seu labia utrinque ad perinæum concurrentia fossulam navicularem densiorem constituerant. Ventriculus cum intestinis, in utrâque, naturaliter erant situata; intestina recta autem utrinque ad os sacrum reflexa et coalita, unum satis amplum et communem canalem constituerunt: os sacrum ad secundam divisionem

concretum erat, et unum corpus efformando, in uno utrique offi sacro communi, offe coccygis, terminabatur.

Ex prærecensitis, sicut causa diversitatis actionum et functionum patet, ita etiam ex arteriarum aortarum, et venarum cavarum, intestinorum quoque rectorum et vaginarum uteri, compagine, coadunatione et harmonia, apparet ratio conformitatis et disparitatis morborum, synthanasiæ, communis nifus egerendæ alvi, possibilis imprægnationis alterutrius, vel fors utriusque virginis, uno eodemque coitu.

Hæc omnia conquisiui et retexui, partim e relationibus fide dignis autoptarum; partim ex ephemeridibus B. Caroli Raygeri, foceri mei, qui, dum viveret, medicum cœnobii dicti ordinarium agebat; partim ex libro cœnobiali, cui B. Vir formulas medicamentorum inscripserat. Dab. Posonii die 3 Julii 1757.

Justus Joannes Torkos,
Eques Pannonius, Medic. Doct. et
Liberæ Regiæ Civitatis Posonien-
sis Physicus ordinarius.

The interval between the reading of this paper before the Royal Society, and the present publication, was occasioned by the long indisposition, and afterwards death, of their late President Martin Folkes, Esq; who having taken it to his house, with a view of collecting and adding to it some further particulars, it could not be found after his decease. But Dr. Torkos, the writer, being again applied to, immediately transmitted the copy of it
printed

printed above: and, in order to supply in some measure the want of what Mr. Folkes's extensive reading and industry might have furnished the public with, in relation to so very remarkable a fact, the following accounts, printed and manuscript, are subjoined as a supplement to the preceding article.

*Extract of a Letter of William Burnet, Esq; F.R.S. eldest Son of Dr. Gilbert Burnet, Lord Bishop of Salisbury, to Dr. (afterwards Sir) Hans Sloane, dated at Leyden, May 9. 1708. N.S. **

“ S I R,

“ **I** Send you inclosed the print of a wonderful
 “ union of two twin sisters, who are at this
 “ time to be seen at the Hague. I saw them, and
 “ observed all, that I could think tended to explain
 “ the appearance. They are Hungarians, as the
 “ lines under the print will shew you. There is
 “ there an exact enough description of their condi-
 “ tion; only I may add, that in fig. 1. the urinal
 “ passage is between the two foremost thighs, as
 “ they are in the print. The same is true of the
 “ anus in the 2d figure, in such manner, that the
 “ situation of these parts is the same to outward ap-
 “ pearance as naturally, with this difference, that
 “ they are between two different bodies here, where-
 “ as in the course of nature they are between the
 “ two parts of the same body. It seems probable,

* Original Letters to Sir Hans Sloane, Bart. vol. A---B. in the British Museum.

" that their parts are distinct; but that the most
 " remote labia of each are outwardly visible, and
 " the two contiguous ones are within. There seems
 " to be no cheat in the thing; and the skin, where
 " they are joined, is perfectly smooth, without any
 " scar. They are now about six years old. They
 " speak French and High German. They are very
 " full of action, and talk one more than the other.
 " When one stoops to take up any thing, she carries
 " the other quite from the ground; and that one
 " of them often does, being stronger as well as more
 " lively than the other. They have not their feeling
 " common any where but in the place of their con-
 " junction. This is all I can say about it. If you
 " think it worth while, you will do me an honour
 " in giving the print, and the substance of this ac-
 " count, to the Society; to which, tho' an unworthy
 " member, I would be proud to be capable of any
 " service."

This letter was read to the Royal Society on the
 12th of May 1708 †; and the print mentioned in
 it produced; which, being now become extremely
 difficult to be met with, is thought proper to be en-
 graved again, and inserted here. See TAB. XIII.

Soon after the date of Mr. Burnet's letter, the
 twin sisters were brought to England, and publicly
 shewn in London, as appears from the following MS.
 note in a copy of the print bound up by the writer
 with Fortunius Licetus *de Monstris* ‡, edit. Am-

† Journal, vol. xi. p. 143.

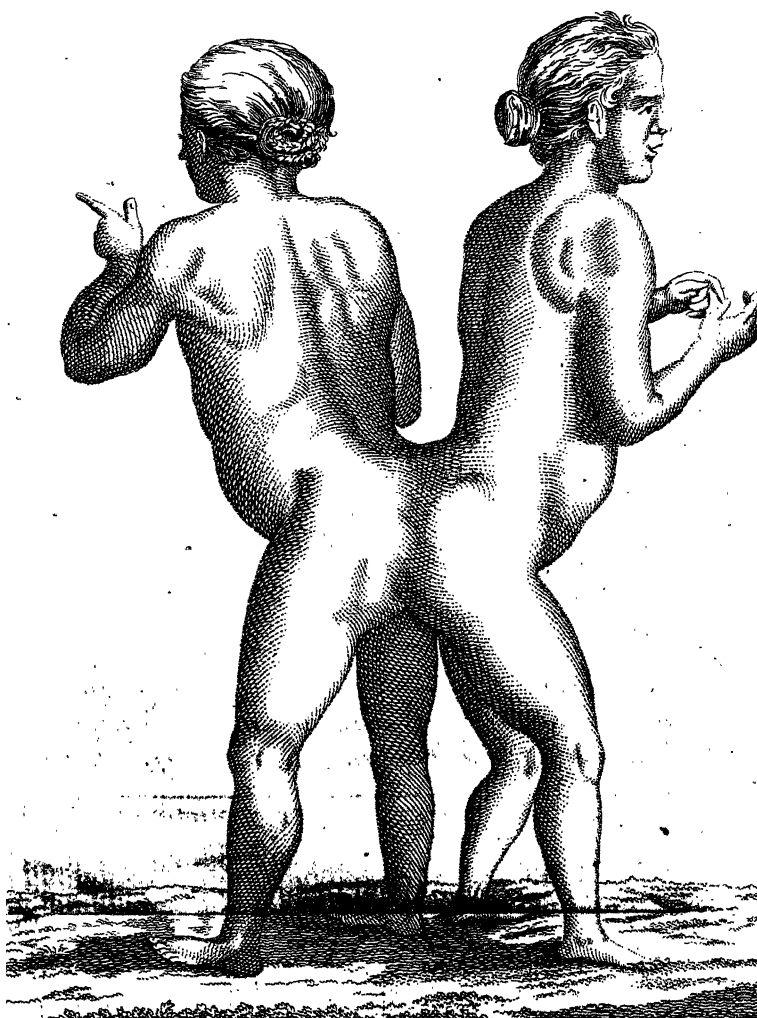
‡ In this treatise, L. 2. p. 80. is the following passage: *In pago Rorbachio non procul Haydelbergâ, Paræi etiam relatu, gemini utriusque sexûs ætatis terribilibus annexis orti sunt.*



*Corporum Sororum sic concrevere Sororum,
Noa, divina dissocianda manu.*

*QVOD EST, VIAUS COMOR, conterminus A,
Quamquam Luce pareat Imperio.*

*HEC EST, VIAUS COMOR, conterminus A,
Quamquam Luce pareat Imperio.*



*Exitus Urinae patet utriusque sinistis alvo,
 Observant humerum dexterae umbra suum.
 Misi ad Ignorantis tenuis, Parentum,
 Neve perca tanta, stupenda rei.
 Interiora Lumbi non, VI Der I:
 Ex. In are patet.*

stelod. 1665. 4to. in the possession of Thomas Wilbraham, M. D. F. R. S. "*Londini 14 Junii 1708. " has vidi gemellas (plus annis sex natas) quarum " forma et vivacitas elegantior et vegetior quam " pictura et descriptio.*"

Another account of them by an eye-witness in London is in a manuscript volume among those of Sir Hans Sloane, Bart. in the British Museum, intituled, *A short History of human Prodigies and monstrous Births, of Dwarfs, Sleepers, Giants, strong Men, Hermaphrodites, numerous Births, and extreme old Age, &c.* The name of the writer was James Paris du Pleffis. In p. 39. under the Title *Two Sisters conjoined*, he gives a drawing of them, and the following description: " These two monstrous " girls were born at Szony in Hungary in the year " 1701. They were born conjoined together " at the " small of the back. I asked the father and mother, " if they could not be separated one from the other? " but they answered, No; because the urinary and " fœcal vessels and passages were so united, as to have " but one issue for the urine, and another for the ex- " crements, betwixt both. They were brisk, merry, " and well-bred: they could read, write, and sing very " prettily: they could speak three different languages, " as Hungarian or High Dutch, Low Dutch, and " French, and were learning English. They were " very handsome, very well shaped in all parts, and " beautiful faces. Helen was born three hours be- " fore her sister Judith. When one stooped, she " lifted the other from the ground, and carried the " other upon her back; neither could they walk " side

“ fide by fide. They loved one another very tenderly. Their clothes were fine and neat. They had two bodies, four sleeves; and one petticoat served to the bodies, and their shifts the same. When one went forward, the other was forced to go backward.”

A later and more particular account is contained in p. 41, & *seqq.* of a book very seldom met with in this country, being printed at Vienna in 1729. intituled, *Gerardi Cornelii Drieschii Historia magnæ Legationis Cæsareæ, quam Caroli VI. auspiciis suscepit Damianus Hugo Virmondcius, &c.* The following extract, tho’ long, will not probably be thought unentertaining.

“ Sunt in comitatu Commaroniensi in terris illustriſſimi Zichii (pagus Hungaris Szony dicitur) à parentibus colonis, quibus sua vita constat, dum hæc scribo, anno 1701. vii calendas Novembreis procreatæ in lucem duæ filiæ gemellæ, posticâ parte, quâ spina dorſi definit, concretæ, sic ut altera alteram sequi, quo se cunque vertat, cogatur : cætera haud deformes aspectu, nisi concretio illa corporum prodigium efficeret. Binæ singulis manus, totidem pedes, et capita, necnon corpora : suus membris omnibus usus ; rationis multò etiam, quod mirere, certior ; ut, si sedentes solum videris, neque sciveris, hic monstri notare nihil valeas. Majori natu, quæ lucem citius aspexit tribus horis, Helenæ, minori Judithæ nomen est. Hæc ante annos circiter tres stupore apoplectico tacta linguæ modicum ex eo ac bonæ mentis officium impeditum habuit, simplicitatem quandam ingenii
“ modo

“ modo ut redoleat. Illa animo semper integro at-
 “ que spiritu prædita eodem, pudicâ facie, non in-
 “ concinnis motibus, intuentium in se oculos ad
 “ misericordiam commovet, utpote quæ rationis
 “ planè compos, sororis tenerrimè amans, nec status
 “ ignara fui, duplicem miseriam tolerat, suam et
 “ istius. Ductæ sunt olim infantes per varias re-
 “ giones ac provincias, Germaniam, Angliam, Gal-
 “ liam, Italiam, Poloniam, Bataviam, Austriam, Mo-
 “ raviam, Hungariam, à medico Hungarico nomine
 “ Csfuzio, qui easdem certâ pecuniâ ad tempus sibi
 “ a parentibus concreditas et elocatas, bonâ eorun-
 “ dem veniâ, paterno à solo âbduxit; unde trium
 “ gentium linguis, Germanicâ, Gallicâ, Hungaricâ,
 “ hodieum etiam loquuntur; alias desuetudine
 “ usuque interrupto, ætate præsertim nondum satis
 “ confirmatâ, omnino dediticere.

“ Dux Augustus Saxo Cizius inter purpuratos
 “ LXXII patres à constantia religionis, timore erga
 “ Deum et caritate in proximum notissimus, archi-
 “ episcopus Strigoniensis, veritus, ne frequentes hæ
 “ perignationes puellarum adhuc infantium inno-
 “ centiæ officerent, ac mores denique illarum, ut
 “ fieri non raro assolet, planè depravarent, pacto
 “ persolutoque pretio à medico redemit, et revocatas
 “ domum ad suos virginibus à divâ Ursulâ nuncu-
 “ patis intra Posonium deinceps educandas commisit,
 “ necessariis ad hoc sumptibus benignè subministratis.
 “ Ingressæ non diu puerilem ætatem fuerant, non us-
 “ que illis annus agi cœptus currebat, quando harum
 “ in disciplinam virginum tradebantur. Illic legere
 “ primum ac scribere, ea, quæ ad fidem necessaria
 “ sunt, mente atque animo comprehendere; operas
 “ manuum

“ manuum exercere varias, acu præcipuè phrygionicâ
 “ pingere, denticulatas affabre fimbrias conficere, et
 “ cætera quæ sunt ejusdem generis, edoctæ fuerunt.
 “ Vidi ego ex illarum operibus aliqua, quæ magis-
 “ tras hâc in arte fecisse non pudeat. Receptæ autem
 “ sunt sacrum hoc in collegium anno secul ix. die xii.
 “ kalendas Aprilis, ibi xi mansionis, vitæ xix jam
 “ planè complerunt. Istuc divarum contubernium,
 “ quod adhuc constanter incolunt, nunquam postea
 “ deseruere. Addita illis è prudentioribus virago,
 “ quæ indefinentur adfit, quo velint, ducat, actioni-
 “ bus invigilet, de quibus respondere, ad aliosque
 “ referre, si necessum fuerit, aliquando possit. Ex
 “ hâc scire quæ cupiebam, remotis arbitris, nullo
 “ negotio percepi. Crediderat namque, quod res
 “ quoque erat, non curiositatis gratiâ, sed officii, ac
 “ boni publici causâ ista à me rogari: quare alios
 “ omnes secedere jussi, solus cum eadem remanens,
 “ ut quæ, junioribus præsertim aliquot præsentibus,
 “ accuratius explicare verecundia illam antea pro-
 “ hibuerat, majori mecum libertate communicaret.
 “ Partes, quas vel nominare pudor honestasque ve-
 “ tuit, per quas potus ciborumque fæces et reliqua
 “ corporis excrementa (sit verbo venia) ejicimus, non
 “ illis his, quibus nobis, constitutæ locis. Illis qui-
 “ dem, ubi nos eas habemus, occlusa sunt omnia;
 “ at internè, quâ parte concretio illa corporum in-
 “ cipit, easdem obtinent utrique communes. Neque
 “ tamen cum necessitas alterutram premit ad exone-
 “ randum, exempli gratiâ, ventrem, altera se quoque
 “ sentit tam inutili pondere gravatam, ut satisfacien-
 “ dum necessario naturæ sit: sed jam huic, jam isti
 “ istud imbecillitatis humanæ incommodum perfe-
 “ rendum

“ rendum est, fitque etiam, ut cum alvum purgat
 “ altera, alteri meatus sit urinarius aperiendus. Mu-
 “ liebria, quæ statis fœminas vicibus incommodant,
 “ non uno ambabus tempore veniunt. Octidui
 “ quandoque intervallo ac longiori disjuncta sunt.
 “ Dum dormit hæc, sæpe vigilat illa, et in alterius
 “ labore altera nonnunquam quiescit. Vifa una po-
 “ tare est, aut cibo corpus reficere, cum aliud alii
 “ agebatur. Sedent, stant, ambulant, jacent semper
 “ unâ, nec sine incommodo. Non permittit con-
 “ glutinatio ista corporum, hæc uti actiones separen-
 “ tur. Si colloquuntur, obtortis faciem collis ob-
 “ vertunt. Suavia dant sibi, cum amant, et pugnis
 “ impetunt, cum furunt. Donec suæ utrique vires
 “ adhuc constabant, si forte exortæ inter illas ali-
 “ quando discordiæ essent, hæc, quæ se læsam magis
 “ credebat aut fortem, sublatam in humeros aliam
 “ alio asportabat. Veruntamen ingenio miti magis ac
 “ placido sunt quam incenso aut iracundo, et in com-
 “ munibus malis communem fidem, commune ro-
 “ bur adhibent, immissam sibi à Deo miseriam for-
 “ titèr sustinentes. Ante triennium in gravi secundò
 “ genitæ morbo, de quo nonnulla superiùs facta est
 “ mentio est, prior nata sacris omnibus munita ad
 “ mortem quoque feliciter obeundam disposita ab
 “ sacerdote fuit, quia medicorum pars potior credit
 “ aliâ extinctâ aliam haud posse longum amplius
 “ superesse. Id quod probare ex hoc etiam laborant,
 “ quod quoties male uni sit, quamvis altera non
 “ eadem continuo ægrotatione teneatur, angustias
 “ tamen animi certas, hebetationem sensuum, et
 “ commotionem quandam viscerum in seipsa ex-
 “ periat. Equidem dubitandum minimè reor, quin
 “

“ monſtroſa hæc bina corpora duplici mente ac ſpi-
 “ ritu regantur. Nam ſive cor faciamus, ſive cere-
 “ brum ſtatuumus animi ſedem, ex utrolibet idem
 “ nullo negotio evincitur. Adde tot actiones multi-
 “ plices, cogitationes rerum diverſas, ſenſa animi varia,
 “ quæ, ut aliud nihil ſit, iſthuc pariter nos docent.
 “ Unum præcipuè hic admirandum venit, quod
 “ commemorare ſuperiùs memoriâ excidit; poſt
 “ prodigioſum videlicet hunc difficilemque partum
 “ natos eſſe matri alios liberos, ex eodem patre pro-
 “ creatos, ſanos et valentes, corpore, ſpecie ac forma
 “ integros, qui monſtri nihil admixtum habeant.”

XL. *Observations on the Origin and Uſe of
 the Lymphatic Veſſels of Animals: being
 an Extract from the Gullſtonian Lectures,
 read in the Theatre of the College of Phy-
 ſicians of London, in June 1755. By
 Mark Akenſide, M.D. Fellow of the College
 of Phyſicians, and of the Royal Society.*

Read Nov. 10,
 1757.

IT is proved, by a multitude of expe-
 riments, that the lymphatics com-
 municate with the blood-veſſels. They may be
 diſtended by blowing air, or by injecting water or
 mercury, into an artery: and the lymph, which
 they carry, is frequently, in a morbid ſtate, found
 tinged with a mixture of the red globules or craſſa-
 mentum of the blood. Upon this foundation two
 different

different theories have been raised, concerning the connection of the lymphatics with the arteries.

Of these, we shall first consider that of the late famous professor Boerhaave. He observed, that every artery of the body is greater, in its diameter, than any of its branches : and this observation being found true, as far as our eye and the microscope can inform us, he inferred, by analogy, that it held good even thro' the most minute subdivisions of the arterial system. But, says he, proportionable to the diameter of the canal is the size of the particles moving thro' it : therefore, if an ultimate capillary artery, admitting only one red globule at once to pass thro' it, send off lateral branches, these branches will be capable of receiving such particles only as are smaller than a red globule. But the particles next in magnitude below the red globules are the yellow serous ones ; and the lateral vessel, thus receiving them, is a serous artery, and the trunk of a second order of vessels. In like manner, this trunk, being continued on thro' many lessening branches, will at last grow so minute, as to admit only one serous globule : its lateral branches, therefore, will receive only such particles as are smaller than the serous ones : but these are the particles of the lymph ; and this lateral branch is a lymphatic artery, and the trunk of a third order of vessels. Thus, in the red arteries are contained all the circulated fluids of the body ; in the serous arteries, all except the red blood ; in the lymphatics, all except the red blood and serum : and this subordination is, according to the same laws, continued down thro' fluids more subtiler than the lymph, to the smallest vessel, which is propagated from the aorta.

Such was Boerhaave's doctrine concerning the vascular system of animal bodies; like many of his other notions, ingenious, plausible, and recommending itself, at first sight, by an appearance of geometrical and mechanical accuracy: but founded upon insufficient data, and by no means to be reconciled to appearances.

For, in the first place, should we admit his hypothesis, it is certain, that the conical or converging form of the aorta, and the change of direction in its branches, must, in the distant blood-vessels, occasion a great resistance to the moving blood, and a great diminution of its velocity. Suppose that this resistance be, in any capillary red artery, to the resistance in the trunk of the aorta, as any larger assignable number is to unit: the resistance, then, in a capillary serous artery will, to that in the aorta, be as the square of that number is to unit; in the capillary lymphatic, as the cube; and so in progression: that is, the velocity of the fluids, in the remoter series of vessels, will be, physically, nothing. But we know, on the contrary, that some very remote series of vessels have their contents moved with a very considerable velocity; particularly the vessels of the insensible perspiration: and in anatomical injections, the liquor thrown into an artery scarce returns more easily or speedily by the corresponding vein, than by the most subtil excretory ducts. Moreover, there are an infinite number of observations of morbid cases, in which the red blood itself has been evacuated thro' some of the most remote series of vessels, merely from an occasional temporary obstruction in one part; or a preternatural laxity in another; and without
any

any lasting detriment to the structure and subordination of the vessels; which yet, upon this hypothesis, must have been utterly destroyed before such an irregularity could have happened.

The other theory concerning the origin of the lymphatics has been maintained by some very eminent physiologists later than Boerhaave; and supposes, that these vessels receive their lymph from the blood-vessels, or from the excretories of the larger glands, by the intermediation of only one small vessel, which these authors term a lymphatic artery, invisible in its natural state, nor yet rendered subject to the senses by experiments. But to this it may be answered, that the lymphatics are traced into many parts of the body, and lost there; and therefore most probably have their origin there, where no large gland nor blood-vessel is to be found in their neighbourhood: that it contradicts the whole analogy of nature, to suppose the motion of an animal fluid more discernible in the veins than in the arteries: and, finally, that it seems rather an instance of want of thought, and of being imposed upon by words, to call the lymphatic vessels veins, because they are furnished with valves; and then, because they are called veins, to take for granted, that of course they must be the continuation of arteries.

In attempting to investigate matters too subtle for the cognizance of our senses, the only method, in which we can reasonably proceed, is by inferring from what we know in subjects of the same nature: and our conclusion thus inferred; concerning the subject sought, will be firmer and more unquestionable, in proportion as it resembles the subject known. But

if the subjects be really of the same kind; if no difference can be shewn between them, in any respect material to the inquiry, in which we are engaged; in this case our inference from analogy becomes the very next thing to a physical certainty: and this I apprehend to be true in relation to the problem before us, concerning the origin of the lymphatic vessels. Tho' in general we cannot, by experiments, arrive at the extremities of those tubes, nor satisfy ourselves, by inspection, in what manner they receive their fluid; yet in a very considerable number of them we can do both. There is a certain part of the human body very abundantly provided with lymphatics; in which part we can actually force injections thro' those vessels into a cavity, where their extremities open: and from this cavity, on the other hand, we can at pleasure introduce a coloured liquor into their extremities, and trace it from smaller into wider canals; from capillary tubes, without valves, into large lymphatic trunks copiously furnished with them. We know likewise, that into this cavity are continually exhaling an infinite number of watery and mucous vessels, both arterial tubes and excretory ducts: that these keep it moist with a perpetual vapour, which the extremities of those lymphatics are, in the mean time, perpetually imbibing. Does it not seem strange, while these particulars are known and acknowledged by all the world, that the great authors of anatomy and physiology should never have reasoned from them; but should run into complex and obscure suppositions, in order to explain a process, which they may at any time examine with their own eyes? But perhaps this inadvertency may be accounted

accounted for, if we recollect, that at the time when these vessels, and the structure of this part, were discovered, the lymph, and every thing belonging to it, was utterly unknown; and that the vessels in question were first seen and considered as performing another and more remarkable office: which circumstance, it should seem, has prevented succeeding authors from being duly attentive to them in the capacity of lymphatics. However this be, it is certain, that the lymphatics of the mesentery, commonly called the lacteals, differ from those of the other parts in no one particular, save that occasionally they carry chyle instead of lymph; or rather carry lymph mixed, at stated times (that is, for two or three hours after the creature has taken food) with an emulsion of vegetable and animal substances, and coloured white by that mixture. At other times, (that is, during sixteen or eighteen hours out of the twenty-four) they contain nothing but lymph; and are, in every respect, mere lymphatic vessels, not to be distinguished from those in any other part of the body. Their structure is the same; the membrane of which they are formed, their valves, the lymph which they contain, the glands thro' which they pass, their direction from smaller tubes to larger, and from these to the blood, differ in nothing from what we observe of the other lymphatics. Their lymph, in the mean time, is without doubt or controversy supplied from the cavity of the intestines; being the watery moisture continually exhaled there for the purposes of digestion, and for the preservation of the alimentary canal, and as continually taken up by the roots or extremities of these vessels, in
order

order to be carried back to the blood, after it has performed its office in the bowels. Let it also be remembered, that these vessels, in other places of the body, are generally, when we trace them, lost in muscular, tendinous, or membranous parts : and then, I should presume, it may fairly, and with a good degree of evidence, be concluded, that the lymphatics of the body, in general, have their origin among the little cavities of the cellular substance of the muscles, among the mucous folliculi of the tendons, or the membranous receptacles and ducts of the larger glands : that their extremities or roots do, from these cavities, imbibe the moisture exhaled there from the ultimate arterial tubes, just as the lacteals (the lymphatics of the mesentery) do on the concave surface of the intestines : and that the minute imbibing vessels, by gradually opening one into another, form at length a lymphatic trunk, furnished with valves to prevent the return of its fluid, and tending uniformly, from the extremities and from the viscera, to reconvey to the blood that lymph, or that fine stream, with which they are kept in perpetual moisture ; a circumstance indispensibly necessary to life and motion : while, at the same time, the continual re-absorption of that moisture by the lymphatics is no less necessary, in order to preserve the blood properly fluid, and to prevent the putrefaction, which would inevitably follow, if this animal vapour were suffered to stagnate in the cavities where it is discharged.

XLI. *A Letter to the Right Honourable the Earl of Macclesfield, President, the Council, and Fellows, of the Royal Society, concerning the Variation of the Magnetic Needle; with a Sett of Tables annexed, which exhibit the Result of upwards of Fifty Thousand Observations, in Six periodic Reviews, from the Year 1700 to the Year 1756, both inclusive; and are adapted to every Five Degrees of Latitude and Longitude in the more frequented Oceans. By William Mountaine and James Dodson, Fellows of the Royal Society.*

Dated London, Nov. 9th. 1757.

S I R S,

Read Nov. 10.
1757.

ON the 20th of March 1755, we presented an address to this illustrious Body, intituled, “ An Attempt to point out, “ in a concise manner, the Advantages which would “ accrue from a periodic Review of the Variation of “ the Magnetic Needle, throughout the known World; “ requesting contributions thereto, by communicating such observations concerning it, as had then “ been lately made, or could be procured from “ correspondents in foreign parts.”

This address was read at the same time, and afterwards honoured with a place in the Transactions, vol. xlviii. part ii. for 1754: which favour we now acknowledge in the most grateful manner; and,

suant to our engagements, beg leave to lay before you some account of the communications received, with a specimen of the uses and applications which we have been enabled to make of those, and other assistances with which we have been indulged.

On application to the Honourable the Commissioners of the Navy, we were obliged with an order of free access to all their masters log-books and journals.

The Directors of the Honourable East India Company granted the like privilege.

The Honourable Committee of the Hudson's Bay Company obliged us with sundry observations, made, and tabulated, by their own Captains.

James Bradley, D.D. Regius Professor of Astronomy, and F.R.S. favoured us with several observations made at the Royal Observatory at Greenwich.

John Hyde, Esq; F.R.S. communicated a set of useful observations, extracted from two journals kept on board the Triton and Britannia East Indiamen.

A correct journal kept on board the Delawar East Indiaman was handed to us by a gentleman unknown.

Capt. George Snow furnished a considerable number of observations, made with care and accuracy by himself, in several successive voyages to, and from Barbadoes and Virginia; together with several remarks upon the subject: *One*, which we apprehend to be material, we beg leave to insert, as it meets with some confirmation by the tables annexed; *viz.*
 " At Barbadoes the variation seems at a stand very
 " near; for in the road, 1752, I observed 5 degrees
 " east; and by Mr. Halley's draught, in the year
 " 1701, $5\frac{1}{2}$ degrees: in 1747, at Port Royal keys,
 " Jamaica, I observed the variation $7^{\circ} 20' E.$; and
 " on the coast of Carthagera the same week, off
 " the

“ the high land of Sancta Martha, $7^{\circ} 45'$ E. nearly
 “ south of Port Royal : Therefore these curves are
 “ not much altered ; and the curve at Jamaica is
 “ nearly at a stand, as tho’ tied ; and the south part
 “ of them, with the rest, dropping to the westward.”

Mr. Mungo Murray, author of a treatise on ship-building, presented us with several observations taken on board the Prince Edward and Chesterfield East Indiamen, and his Majesty’s ship the Neptune.

For all these favours we return our sincere thanks.

No observations made upon land have been received, except Dr. Bradley’s aforesaid ; which has frustrated our intentions of continuing the curves from sea to sea.

By collecting, comparing, and adjusting, all these materials, we have been enabled to construct variation-curves upon Dr. Halley’s mercator-chart, adapted to the year 1756 ; which will soon be in readiness to present to this Royal Society.

As a work of this kind requires much time, and a multitude of observations, both by sea and land, to render it more perfect and general ; we hope the ingenious in all nations will lend their assistance : By this means every periodic review will be productive of improvement.

From the first instant that we made this affair the object of our more particular consideration, we have attended to the mode of increase and decrease in the variation : and as a considerable number of observations, made at periodic times, and duly registred, seem to be the most essential toward determining the laws of its mutation, or proving its irregularity, we have therefore formed a sett of tables, from actual observations collected for the years 1710, 1720, 1730, and 1744, the date of our last chart ; which, together

with Dr. Halley's for the year 1700, and the present chart now publishing, compleat six reviews: These are tabulated, and shew the quantity of the variation, at those several periods, to every 5 degrees of latitude and longitude in the more frequented oceans; which we hope will prove acceptable, as nothing of the like kind has yet appeared, or can easily be obtained.

Our materials have been so deficient, that even in the limits to which our tables are confined, we have been obliged to leave blanks in some of the above periods, for want of that concurrent testimony, on which the numbers inserted are founded: but, considering the difficulties unavoidably attending a work of this sort, and the little assistance which we have met with from private hands, we hope that this Royal Society will not only excuse those vacancies, but also those in the great tracts of sea, as well as land, concerning which we are very unwillingly obliged to be intirely silent.

Agreeable to our former address, we lay only what appear to be facts before you, without attempting to introduce any hypothesis for the solution of these phenomena; some of which (being very extraordinary) we recommend peculiarly to the notice of those gentlemen, who may endeavour the investigation of their causes.

Under the equator, in longitude 40° E. from London, the highest variation during the whole 56 years appears to be $17^{\circ} \frac{1}{4}$ W. and the least $16^{\circ} \frac{1}{2}$ W.: and in latitude 15° N. longitude 60° W. from London, the variation has been constantly 5° E. but in other places the case has been widely different; for in the latitude 10° S. longitude 60° E. from London, the variation has decreased from 17° W. to $7^{\circ} \frac{1}{2}$ W.; and in latitude 10° S. longitude 5° W. from London, it
has

has increased from $2^{\circ}\frac{1}{4}$ W. to $12^{\circ}\frac{3}{4}$ W.; and in latitude 15° N. longitude 20° W. it has increased from 1° W. to 9° W.

But there is still a more extraordinary appearance in the Indian seas : for instance, under the equator.

Longitude from London	Variation in		Longitude from London	Variation in	
	1700.	1756.		1700.	1756.
Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.
40 E	$16\frac{3}{4}$ W	$16\frac{3}{4}$ W	75 E	$9\frac{3}{4}$ W	1 W
45 E	$17\frac{3}{4}$ W	$14\frac{1}{2}$ W	80 E	$7\frac{3}{4}$ W	$0\frac{1}{4}$ E
50 E	$17\frac{1}{2}$ W	$11\frac{3}{4}$ W	85 E	$5\frac{1}{2}$ W	$1\frac{1}{4}$ E
55 E	$16\frac{1}{2}$ W	$8\frac{1}{4}$ W	90 E	$4\frac{1}{4}$ W	1 E
60 E	$15\frac{1}{4}$ W	6 W	95 E	$3\frac{1}{4}$ W	$0\frac{1}{2}$ W
65 E	$13\frac{1}{2}$ W	$4\frac{1}{2}$ W	100 E	$2\frac{1}{2}$ W	1 W
70 E	$11\frac{1}{2}$ W	$3\frac{1}{4}$ W			

Where the west variation in the longitude 40° E. is the same in both the above years ; and in 1700 the west variation seemed to be regularly decreasing from longitude 50° E. to the longitude 100° E. ; but in 1756 we find the west variation decreasing so fast, that we have east variation in the longitude 80° , 85° , and 90° E ; and yet, in the longitude 95° and 100° E. we have west variation again.

Such are the irregularities, that experience hath shewn us, in the variation of the magnetic needle ; which appear so considerable, that we cannot think it wholly under the direction of one general and uniform law ; but rather conclude, with the learned and judicious Dr. Gowen Knight, Fellow of this Society, in the 87th prop. of his treatise upon attraction and repulsion, That it is influenced by various and different magnetic

magnetic attractions, in all probability occasioned by the heterogeneous compositions in the great magnet, the *Earth*.

Notwithstanding all which, should the sagacity of some eminent philosopher be able to exhibit rules, whereby the quantity of the variation may be computed for future times, yet then such a review, as we have now made, will be necessary at a proper interval, to prove the truth of them : and should no such rules appear, then will a continued succession of such reviews be necessary so long as commerce and navigation subsist among us.

What we have now done is intirely for the public service, the sale of the former chart never having made good its expence ; and we propose to continue our endeavours for another review, at the proper time, if we shall then be alive, and capable of the task : but as the contrary may probably happen, we beg leave to conclude with recommending such a continuation, in the strongest manner, to such of the members of this Royal Society, or others, who may, at the proper intervals, have leisure and ability for such a performance.

We are, with the greatest deference,

Your Lordship's,

And the Royal Society's,

Most faithful and

most obedient Servants,

William Mountaine.

James Dodson.

A TABLE, exhibiting the different Variations of the MAGNETIC-NEEDLE in the more frequented Oceans, from the Year 1700 to the Year 1756.

Latitude.	Longitude, From London.	V A R I A T I O N.					
		Anno 1700.	Anno 1710.	Anno 1720.	Anno 1730.	Anno 1744.	Anno 1756.
Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.
0	0	4 $\frac{1}{2}$ W			10 $\frac{1}{4}$ W	14 $\frac{1}{4}$ W	15 $\frac{1}{4}$ W
0	5 W	2 $\frac{3}{4}$ W			8 $\frac{3}{4}$ W	12 W	13 $\frac{1}{4}$ W
0	10 W	1 $\frac{1}{2}$ W	3 $\frac{1}{2}$ W	5 $\frac{1}{2}$ W	7 W	10 W	11 W
0	15 W	0 $\frac{1}{2}$ W	2 W	3 $\frac{1}{2}$ W	5 W	7 $\frac{1}{2}$ W	9 W
0	20 W	0 $\frac{3}{4}$ E	0 $\frac{1}{2}$ W	1 $\frac{3}{4}$ W	3 W	5 $\frac{1}{4}$ W	6 $\frac{1}{2}$ W
0	25 W	1 $\frac{1}{2}$ E	0 $\frac{1}{2}$ E	0 $\frac{1}{2}$ W	1 $\frac{1}{2}$ W	3 W	4 W
0	30 W	2 $\frac{1}{2}$ E	2 E	1 $\frac{1}{2}$ E	1 E	0 $\frac{3}{4}$ W	0 $\frac{1}{2}$ W
0	35 W	3 $\frac{1}{4}$ E	3 E	2 $\frac{3}{4}$ E	2 $\frac{1}{2}$ E	1 $\frac{1}{2}$ E	1 $\frac{1}{2}$ E
0	40 W	4 $\frac{1}{4}$ E	4 $\frac{1}{4}$ E	4 E	3 $\frac{3}{4}$ E	3 $\frac{1}{2}$ E	3 $\frac{1}{2}$ E
0	45 W	5 $\frac{1}{2}$ E	5 $\frac{1}{2}$ E	5 $\frac{1}{4}$ E	5 E	4 $\frac{3}{4}$ E	5 E
0	50 W	6 $\frac{1}{4}$ E			5 $\frac{1}{4}$ E	6 E	6 $\frac{1}{2}$ E
0	5 E	6 W			12 $\frac{1}{2}$ W	15 $\frac{1}{4}$ W	16 $\frac{1}{2}$ W
0	10 E	7 W			14 $\frac{1}{4}$ W	17 W	17 $\frac{1}{2}$ W
0	40 E	16 $\frac{1}{4}$ W	17 W	17 $\frac{1}{4}$ W	17 W	16 $\frac{1}{2}$ W	16 $\frac{1}{4}$ W
0	45 E	17 $\frac{1}{4}$ W	17 $\frac{1}{4}$ W	16 $\frac{3}{4}$ W	16 $\frac{1}{4}$ W	15 $\frac{3}{4}$ W	14 $\frac{1}{2}$ W
0	50 E	17 $\frac{1}{2}$ W	16 $\frac{1}{4}$ W	16 W	15 W	14 W	11 $\frac{1}{4}$ W
0	55 E	16 $\frac{1}{2}$ W	15 $\frac{1}{4}$ W	14 W	13 W	11 $\frac{1}{2}$ W	8 $\frac{3}{4}$ W
0	60 E	15 $\frac{1}{4}$ W	13 $\frac{3}{4}$ W	12 $\frac{1}{2}$ W	11 W	9 W	6 W
0	65 E	13 $\frac{1}{2}$ W	11 $\frac{1}{4}$ W	10 W	8 $\frac{1}{4}$ W	6 $\frac{1}{2}$ W	4 $\frac{1}{2}$ W
0	70 E	11 $\frac{1}{2}$ W	9 $\frac{1}{4}$ W	7 $\frac{3}{4}$ W	6 W	4 W	3 $\frac{1}{4}$ W
0	75 E	9 $\frac{1}{4}$ W	7 $\frac{1}{4}$ W	5 $\frac{3}{4}$ W	4 W	1 $\frac{3}{4}$ W	1 W
0	80 E	7 $\frac{1}{4}$ W	6 W	4 $\frac{1}{4}$ W	3 W	0 $\frac{1}{4}$ W	0 $\frac{1}{4}$ E
0	85 E	5 $\frac{1}{2}$ W	4 W	2 $\frac{1}{2}$ W	1 $\frac{1}{2}$ W	0 $\frac{1}{4}$ E	1 $\frac{1}{4}$ E

VARIATION.

Lati- tude.	Longi- tude, From London.	Anno 1700.	Anno 1710.	Anno 1720.	Anno 1730.	Anno 1744.	Anno 1756.
Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.
0	90 E	4 $\frac{1}{4}$ W	3 $\frac{1}{2}$ W	1 $\frac{1}{2}$ W	0 $\frac{1}{2}$ W	1 $\frac{1}{4}$ W	1 E
0	95 E	3 $\frac{3}{4}$ W	2 $\frac{1}{4}$ W			2 W	0 $\frac{1}{2}$ W
0	100 E	2 $\frac{1}{2}$ W	1 W			2 $\frac{1}{2}$ W	1 W
5 N	0	4 $\frac{3}{4}$ W			10 $\frac{1}{2}$ W	15 W	15 $\frac{1}{4}$ W
5 N	5 W	3 $\frac{3}{4}$ W			9 W	13 W	13 W
5 N	10 W	1 $\frac{3}{4}$ W			7 $\frac{1}{2}$ W	10 $\frac{3}{4}$ W	11 $\frac{1}{4}$ W
5 N	15 W	0 $\frac{3}{4}$ W	2 $\frac{1}{4}$ W	3 $\frac{3}{4}$ W	5 $\frac{1}{2}$ W	8 $\frac{1}{2}$ W	9 $\frac{1}{4}$ W
5 N	20 W	0	1 $\frac{1}{2}$ W	2 $\frac{1}{2}$ W	3 $\frac{1}{2}$ W	6 W	7 $\frac{1}{4}$ W
5 N	25 W	1 E	0	1 W	2 W	3 $\frac{3}{4}$ W	4 $\frac{1}{2}$ W
5 N	30 W	1 $\frac{3}{4}$ E	1 $\frac{1}{4}$ E	0 $\frac{1}{2}$ E	0 $\frac{1}{4}$ W	1 $\frac{1}{2}$ W	1 $\frac{1}{2}$ W
5 N	35 W	2 $\frac{1}{2}$ E	2 $\frac{1}{4}$ E	1 $\frac{3}{4}$ E	1 $\frac{1}{2}$ E	0 $\frac{1}{2}$ E	0 $\frac{1}{2}$ E
5 N	40 W	3 $\frac{1}{2}$ E	3 $\frac{1}{4}$ E	3 E	2 $\frac{1}{2}$ E	2 $\frac{1}{4}$ E	2 $\frac{1}{4}$ E
5 N	45 W	4 $\frac{1}{2}$ E	4 $\frac{1}{4}$ E	4 $\frac{1}{4}$ E	4 E	3 $\frac{3}{4}$ E	4 E
5 N	50 W	5 $\frac{1}{2}$ E	5 $\frac{1}{2}$ E	5 $\frac{1}{4}$ E	5 $\frac{1}{4}$ E	5 E	5 $\frac{1}{4}$ E
5 N	55 W	6 $\frac{3}{4}$ E				6 $\frac{1}{4}$ E	6 $\frac{1}{2}$ E
5 N	5 E	6 $\frac{1}{4}$ W			12 $\frac{1}{4}$ W	16 $\frac{1}{4}$ W	16 $\frac{1}{2}$ W
5 N	10 E	7 $\frac{3}{4}$ W			14 $\frac{1}{4}$ W	17 $\frac{1}{2}$ W	17 $\frac{1}{2}$ W
5 N	45 E	16 $\frac{3}{4}$ W	16 W	15 $\frac{1}{4}$ W	14 $\frac{3}{4}$ W	14 W	13 $\frac{1}{2}$ W
5 N	50 E	16 $\frac{3}{4}$ W	15 $\frac{1}{2}$ W	14 $\frac{3}{4}$ W	13 $\frac{1}{2}$ W	12 $\frac{1}{4}$ W	11 W
5 N	55 E	15 $\frac{1}{2}$ W	14 $\frac{1}{4}$ W	13 W	12 W	10 W	8 $\frac{1}{4}$ W
5 N	60 E	14 $\frac{1}{4}$ W	12 $\frac{3}{4}$ W	11 $\frac{1}{4}$ W	10 W	8 $\frac{1}{2}$ W	6 W
5 N	65 E	12 $\frac{3}{4}$ W	11 W	9 $\frac{1}{2}$ W	8 W	6 W	4 $\frac{1}{2}$ W
5 N	70 E	10 $\frac{3}{4}$ W	9 W	7 $\frac{1}{4}$ W	5 $\frac{1}{2}$ W	3 $\frac{1}{2}$ W	2 $\frac{1}{2}$ W
5 N	75 E	8 $\frac{3}{4}$ W	7 W	5 $\frac{1}{4}$ W	3 $\frac{3}{4}$ W	1 $\frac{1}{4}$ W	0 $\frac{3}{4}$ W
5 N	80 E	6 $\frac{1}{2}$ W	5 W	3 $\frac{1}{2}$ W	2 $\frac{1}{2}$ W	0 $\frac{1}{4}$ W	0 $\frac{1}{4}$ E
5 N	85 E	4 $\frac{1}{4}$ W	3 $\frac{1}{4}$ W	2 $\frac{3}{4}$ W	1 $\frac{1}{2}$ W	0 $\frac{1}{4}$ W	1 $\frac{1}{4}$ E
5 N	90 E	3 $\frac{3}{4}$ W				1 $\frac{1}{4}$ W	0 $\frac{1}{2}$ E
5 N	95 E	2 $\frac{3}{4}$ W				2 $\frac{1}{4}$ W	0 $\frac{1}{2}$ W

V A R I A T I O N.							
Lati- tude.	Longi- tude, From London.	Anno 1700.	Anno 1710.	Anno 1720.	Anno 1730.	Anno 1744.	Anno 1756.
Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.
10 N	15 W	1 $\frac{1}{4}$ W		4 $\frac{1}{2}$ W	6 W	9 $\frac{1}{4}$ W	10 W
10 N	20 W	0 $\frac{1}{2}$ W	2 W	3 $\frac{1}{4}$ W	4 $\frac{1}{2}$ W	7 W	8 W
10 N	25 W	0 $\frac{1}{4}$ E	0 $\frac{1}{4}$ W	1 $\frac{3}{4}$ W	2 $\frac{1}{4}$ W	4 $\frac{1}{2}$ W	5 $\frac{1}{2}$ W
10 N	30 W	1 E	0 $\frac{1}{2}$ E	0	1 W	2 $\frac{1}{4}$ W	3 W
10 N	35 W	1 $\frac{1}{4}$ E	1 $\frac{1}{4}$ E	0 $\frac{1}{4}$ E	0 $\frac{1}{4}$ E	0 $\frac{1}{2}$ W	1 W
10 N	40 W	2 $\frac{1}{2}$ E	2 $\frac{1}{4}$ E	2 E	1 $\frac{1}{2}$ E	1 $\frac{1}{4}$ E	1 E
10 N	45 W	3 $\frac{1}{2}$ E	3 $\frac{1}{2}$ E	3 $\frac{1}{4}$ E	3 E	2 $\frac{3}{4}$ E	2 $\frac{1}{2}$ E
10 N	50 W	4 $\frac{1}{2}$ E	4 $\frac{1}{2}$ E	4 $\frac{1}{4}$ E	4 $\frac{1}{4}$ E	4 E	4 E
10 N	55 W	5 $\frac{1}{2}$ E	5 $\frac{1}{2}$ E	5 $\frac{1}{2}$ E	5 $\frac{1}{2}$ E	5 $\frac{1}{4}$ E	5 $\frac{1}{2}$ E
10 N	60 W	6 $\frac{1}{2}$ E				6 $\frac{1}{2}$ E	6 $\frac{3}{4}$ E
10 N	50 E	16 W	15 W	14 W	12 $\frac{3}{4}$ W	11 $\frac{1}{4}$ W	10 $\frac{1}{2}$ W
10 N	55 E	15 W	13 $\frac{1}{2}$ W	12 W	11 W	9 $\frac{3}{4}$ W	8 W
10 N	60 E	13 $\frac{1}{2}$ W	12 W	10 $\frac{1}{2}$ W	9 $\frac{1}{2}$ W	7 $\frac{3}{4}$ W	6 W
10 N	65 E	12 W	10 $\frac{1}{2}$ W	9 W	7 $\frac{1}{2}$ W	6 W	4 $\frac{1}{2}$ W
10 N	70 E	10 W	8 $\frac{3}{4}$ W	6 $\frac{1}{2}$ W	5 W	3 $\frac{1}{2}$ W	3 W
10 N	75 E	8 W	6 $\frac{1}{2}$ W	5 W	3 $\frac{1}{2}$ W	1 $\frac{1}{2}$ W	1 E
10 N	80 E	5 $\frac{1}{2}$ W	4 $\frac{1}{2}$ W	3 $\frac{1}{4}$ W	2 W	0 $\frac{1}{2}$ W	0 $\frac{1}{4}$ E
10 N	85 E	4 $\frac{1}{2}$ W	3 $\frac{1}{2}$ W	2 $\frac{1}{2}$ W	1 $\frac{1}{2}$ W	0	1 E
10 N	90 E	3 $\frac{1}{2}$ W				1 $\frac{1}{2}$ W	0 $\frac{1}{2}$ E
10 N	95 E	2 $\frac{1}{2}$ W				2 $\frac{1}{4}$ W	0 $\frac{1}{2}$ W
15 N	20 W	1 W	2 $\frac{1}{4}$ W	4 W	5 $\frac{1}{4}$ W	7 W	9 W
15 N	25 W	0 $\frac{1}{2}$ W	1 $\frac{1}{2}$ W	2 $\frac{1}{4}$ W	3 $\frac{1}{4}$ W	4 $\frac{1}{4}$ W	6 $\frac{1}{2}$ W
15 N	30 W	0 $\frac{1}{4}$ E	0 $\frac{1}{4}$ W	1 W	1 $\frac{1}{2}$ W	2 $\frac{1}{2}$ W	4 $\frac{1}{2}$ W
15 N	35 W	1 E	0 $\frac{1}{2}$ E	0	0 $\frac{1}{2}$ W	1 $\frac{1}{4}$ W	3 $\frac{1}{2}$ W
15 N	40 W	1 $\frac{1}{2}$ E	1 $\frac{1}{4}$ E	1 E	0 $\frac{1}{2}$ E	0	0 $\frac{1}{2}$ W
15 N	45 W	2 $\frac{1}{4}$ E	2 $\frac{1}{4}$ E	2 E	1 $\frac{1}{4}$ E	1 $\frac{1}{2}$ E	1 E
15 N	50 W	3 $\frac{1}{4}$ E	3 $\frac{1}{4}$ E	3 $\frac{1}{4}$ E	3 E	2 $\frac{1}{2}$ E	2 $\frac{1}{2}$ E
15 N	55 W	4 E	4 E	4 E	4 E	4 E	3 $\frac{1}{2}$ E

V A R I A T I O N.							
Lati- tude.	Longi- tude. From London.	Anno 1700.	Anno 1710.	Anno 1720.	Anno 1730.	Anno 1744.	Anno 1756.
Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.
15 N	60 W	5 E	5 E	5 E	5 E	5 E	5 E
15 N	65 W	6 E			5 E	6 E	6 E
15 N	70 W	7 E			5½ E	6¾ E	7 E
15 N	75 W	7¾ E				7 E	7¾ E
15 N	80 W	8¼ E				7 E	8 E
15 N	50 E	15½ W	14¼ W	13 W	11¾ W	10½ W	9¾ W
15 N	55 E	14¼ W	12¾ W	11½ W	10¾ W	8¾ W	7¾ W
15 N	60 E	13 W	11¾ W	10¾ W	9 W	7½ W	6 W
15 N	65 E	11½ W	10 W	8¾ W	7½ W	6 W	4½ W
15 N	70 E	9¾ W	8¼ W	6¾ W	5½ W	4 W	2¾ W
15 N	75 E	8 W	6½ W	5 W	3½ W	2¾ W	0¾ W
15 N	80 E	5¾ W	4½ W	3½ W	2½ W	1½ W	0
15 N	85 E	4¼ W	3¾ W	2¾ W	1 W	0¾ W	0¾ E
15 N	90 E	3¼ W	2½ W	1¼ W		0¾ W	0½ E
15 N	95 E	2½ W	1½ W			2¼ W	0½ W
20 N	20 W	1½ W	3 W	4¼ W	5¼ W	7 W	10 W
20 N	25 W	1 W	2 W	2¾ W	3½ W	4¾ W	8 W
20 N	30 W	0½ W	1 W	1½ W	2¼ W	3¼ W	5¾ W
20 N	35 W	0¼ E	0¼ W	0¾ W	1½ W	2¼ W	4 W
20 N	40 W	0¾ E	¼ E	0	0½ W	1 W	2½ W
20 N	45 W	1½ E	1¼ E	1 E	¾ E	0¾ E	0¾ W
20 N	50 W	2 E	2 E	1¾ E	1½ E	1½ E	0¾ E
20 N	55 W	2¾ E	2¾ E	2¾ E	2½ E	2½ E	2 E
20 N	60 W	3¾ E	3¾ E	3¾ E	3½ E	3½ E	3¼ E
20 N	65 W	4¾ E				4¼ E	4 E
20 N	70 W	5½ E				5 E	5 E
20 N	75 W	6¼ E				5¾ E	5¾ E
20 N	80 W	7 E				5¾ E	6 E

VARIATION.

Lati- tude.	Longi- tude, From London	Anno 1700.	Anno 1710.	Anno 1720.	Anno 1730.	Anno 1744.	Anno 1756.
Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.
20 N	60 E	12 $\frac{3}{4}$ W	11 $\frac{1}{4}$ W	10 W	9 W	7 $\frac{1}{2}$ W	6 W
20 N	65 E	11 $\frac{1}{2}$ W	10 W	8 $\frac{1}{2}$ W	7 $\frac{1}{4}$ W	6 W	4 $\frac{1}{2}$ W
20 N	70 E	9 $\frac{3}{4}$ W	8 $\frac{1}{2}$ W	7 W	5 $\frac{3}{4}$ W	4 $\frac{1}{2}$ W	2 $\frac{3}{4}$ W
20 N	90 E	3 $\frac{1}{2}$ W	2 $\frac{1}{2}$ W	1 $\frac{1}{2}$ W	1 W	0 $\frac{1}{2}$ W	1 E
25 N	20 W	2 W	3 $\frac{1}{4}$ W	4 W	5 $\frac{1}{4}$ W	7 W	11 W
25 N	25 W	1 $\frac{3}{4}$ W	2 $\frac{1}{4}$ W	2 $\frac{3}{4}$ W	4 W	5 $\frac{1}{4}$ W	9 $\frac{1}{2}$ W
25 N	30 W	1 $\frac{1}{4}$ W	1 $\frac{3}{4}$ W	2 $\frac{1}{4}$ W	3 W	4 W	7 $\frac{1}{2}$ W
25 N	35 W	1 W	1 $\frac{1}{2}$ W	2 W	2 $\frac{1}{2}$ W	3 W	5 $\frac{1}{2}$ W
25 N	40 W	0 $\frac{1}{2}$ W	0 $\frac{3}{4}$ W	1 W	1 $\frac{1}{2}$ W	2 $\frac{1}{4}$ W	3 $\frac{3}{4}$ W
25 N	45 W	0 $\frac{1}{4}$ E	0	0 $\frac{1}{2}$ W	1 W	1 $\frac{1}{2}$ W	2 $\frac{1}{2}$ W
25 N	50 W	0 $\frac{3}{4}$ E	0 $\frac{1}{2}$ E	0 $\frac{1}{4}$ E	0	0 $\frac{1}{2}$ W	1 W
25 N	55 W	1 $\frac{1}{4}$ E	1 E	1 E	$\frac{1}{4}$ E	0 $\frac{1}{2}$ E	0
25 N	60 W	2 E	2 E	2 E	1 $\frac{1}{2}$ E	1 $\frac{1}{4}$ E	1 $\frac{1}{4}$ E
25 N	65 W	2 $\frac{1}{4}$ E				2 $\frac{1}{4}$ E	2 $\frac{1}{4}$ E
25 N	70 W	3 $\frac{1}{4}$ E				3 $\frac{1}{4}$ E	2 $\frac{3}{4}$ E
25 N	75 W	4 $\frac{1}{4}$ E				3 $\frac{1}{2}$ E	3 E
25 N	80 W	4 $\frac{3}{4}$ E				3 $\frac{3}{4}$ E	3 E
25 N	60 E	12 $\frac{3}{4}$ W	11 $\frac{1}{2}$ W	10 $\frac{1}{4}$ W	9 W	7 $\frac{1}{2}$ W	6 W
25 N	65 E	11 $\frac{1}{2}$ W	10 $\frac{3}{4}$ W	9 W	7 $\frac{1}{2}$ W	6 W	4 $\frac{1}{2}$ W
25 N	70 E	10 W	8 $\frac{3}{4}$ W	7 $\frac{1}{2}$ W	6 W	4 $\frac{1}{2}$ W	2 $\frac{3}{4}$ W
30 N	10 W	3 $\frac{1}{2}$ W				11 $\frac{1}{4}$ W	13 $\frac{1}{4}$ W
30 N	15 W	3 $\frac{1}{4}$ W				10 $\frac{1}{4}$ W	12 $\frac{1}{4}$ W
30 N	20 W	3 W	4 $\frac{1}{4}$ W	5 $\frac{1}{4}$ W	6 $\frac{1}{4}$ W	8 $\frac{1}{4}$ W	2 W
30 N	25 W	2 $\frac{1}{4}$ W	3 $\frac{1}{2}$ W	4 $\frac{1}{4}$ W	5 $\frac{1}{4}$ W	6 $\frac{1}{4}$ W	0 $\frac{3}{4}$ W
30 N	30 W	2 $\frac{1}{2}$ W	3 W	3 $\frac{1}{2}$ W	4 $\frac{1}{4}$ W		9 W
30 N	35 W	2 $\frac{3}{4}$ W	2 $\frac{3}{4}$ W	3 $\frac{1}{2}$ W	3 $\frac{3}{4}$ W		7 W
30 N	40 W	2 W	2 W	2 $\frac{1}{2}$ W	3 W	3 $\frac{1}{2}$ W	5 $\frac{1}{2}$ W
30 N	45 W	1 $\frac{1}{2}$ W	1 $\frac{1}{2}$ W	2 W	2 $\frac{1}{4}$ W	2 $\frac{1}{2}$ W	4 W

V A R I A T I O N .

Lati- tude.	Longi- tude, From London.	Anno 1700.	Anno 1710.	Anno 1720.	Anno 1730.	Anno 1744.	Anno 1756.
Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.
30 N	50 W	0 $\frac{3}{4}$ W	1 W	1 $\frac{1}{2}$ W	1 $\frac{3}{4}$ W	2 $\frac{1}{4}$ W	3 W
30 N	55 W	0 $\frac{1}{4}$ W	$\frac{1}{2}$ W	1 W	1 $\frac{1}{4}$ W	1 $\frac{1}{2}$ W	2 $\frac{1}{4}$ W
30 N	60 W	0 $\frac{1}{4}$ E	0	$\frac{1}{4}$ W	0 $\frac{1}{2}$ W	1 W	1 $\frac{1}{2}$ W
30 N	65 W	0 $\frac{3}{4}$ E	$\frac{1}{2}$ E	$\frac{1}{4}$ E	0	$\frac{1}{4}$ W	1 W
30 N	70 W	1 $\frac{1}{2}$ E	1 $\frac{1}{4}$ E	1 E	$\frac{1}{4}$ E	$\frac{1}{4}$ E	$\frac{1}{2}$ W
30 N	75 W	2 E	1 $\frac{1}{2}$ E	1 $\frac{1}{4}$ E	1 E	$\frac{1}{2}$ E	0
30 N	80 W	2 $\frac{1}{4}$ E				$\frac{3}{4}$ E	0
35 N	10 W	4 $\frac{1}{4}$ W			9 $\frac{1}{4}$ W	12 $\frac{1}{4}$ W	14 $\frac{1}{4}$ W
35 N	15 W	4 W			9 $\frac{1}{4}$ W	11 $\frac{1}{2}$ W	13 $\frac{1}{4}$ W
35 N	20 W	4 W			8 $\frac{1}{4}$ W	10 $\frac{1}{4}$ W	13 W
35 N	25 W	3 $\frac{3}{4}$ W	4 $\frac{3}{4}$ W	6 W	7 $\frac{1}{2}$ W	9 W	12 $\frac{1}{4}$ W
35 N	30 W	3 $\frac{3}{4}$ W	4 $\frac{1}{2}$ W	5 $\frac{1}{2}$ W	6 $\frac{3}{4}$ W	8 W	10 $\frac{1}{2}$ W
35 N	35 W	3 $\frac{3}{4}$ W	4 $\frac{1}{4}$ W	5 $\frac{1}{4}$ W	6 W	7 W	8 $\frac{3}{4}$ W
35 N	40 W	3 $\frac{3}{4}$ W	4 W	4 $\frac{1}{2}$ W	5 $\frac{1}{4}$ W	6 $\frac{1}{4}$ W	7 $\frac{1}{4}$ W
35 N	45 W	3 $\frac{1}{2}$ W	3 $\frac{3}{4}$ W	4 $\frac{1}{4}$ W	4 $\frac{3}{4}$ W	5 $\frac{1}{2}$ W	6 $\frac{1}{4}$ W
35 N	50 W	3 $\frac{1}{2}$ W	3 $\frac{3}{4}$ W	4 W	4 $\frac{1}{4}$ W	5 W	5 $\frac{1}{2}$ W
35 N	55 W	3 $\frac{1}{2}$ W	3 $\frac{3}{4}$ W	4 W	4 $\frac{1}{4}$ W	4 $\frac{3}{4}$ W	5 W
35 N	60 W	3 $\frac{1}{4}$ W	3 $\frac{3}{4}$ W	4 W	4 $\frac{1}{4}$ W	5 W	5 $\frac{1}{4}$ W
35 N	65 W	3 W				5 $\frac{1}{4}$ W	6 W
35 N	70 W	2 $\frac{1}{2}$ W				5 $\frac{1}{4}$ W	6 $\frac{1}{2}$ W
35 N	75 W	2 $\frac{1}{4}$ W				6 $\frac{1}{2}$ W	7 W
40 N	10 W	5 W			10 $\frac{1}{4}$ W	13 $\frac{1}{4}$ W	15 W
40 N	15 W	5 $\frac{1}{4}$ W			10 $\frac{1}{2}$ W	12 $\frac{1}{4}$ W	14 $\frac{1}{4}$ W
40 N	20 W	5 $\frac{1}{4}$ W			10 W	12 $\frac{1}{4}$ W	14 $\frac{1}{4}$ W
40 N	25 W	5 $\frac{1}{2}$ W			9 $\frac{1}{2}$ W	11 $\frac{1}{4}$ W	13 $\frac{1}{2}$ W
40 N	30 W	5 $\frac{1}{2}$ W	6 $\frac{1}{4}$ W	8 W	9 W	10 $\frac{1}{4}$ W	12 $\frac{1}{4}$ W
40 N	35 W	5 $\frac{1}{4}$ W	6 $\frac{1}{2}$ W	7 $\frac{1}{4}$ W	8 $\frac{1}{4}$ W	9 $\frac{1}{2}$ W	11 $\frac{1}{2}$ W
40 N	40 W	5 $\frac{1}{2}$ W	6 $\frac{1}{4}$ W	7 $\frac{1}{4}$ W	8 W	9 W	10 W

VARIATION.

Lati- tude.	Longi- tude, From London.	Anno 1700.	Anno 1710.	Anno 1720.	Anno 1730.	Anno 1744.	Anno 1756.
Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.
40 N	45 W	6 W			7½ W	8½ W	9½ W
40 N	50 W	6¼ W			7¾ W	8½ W	9½ W
40 N	55 W	6½ W			7¾ W	8¾ W	10 W
40 N	60 W	6¾ W			8 W	9¼ W	11 W
40 N	65 W	7 W			8½ W	10¼ W	12 W
40 N	70 W	7 W			9 W	11½ W	12¾ W
45 N	5 W	6 W			12½ W	15½ W	16½ W
45 N	10 W	6½ W			12¾ W	15 W	16½ W
45 N	15 W	6¾ W			11¾ W	14¾ W	16½ W
45 N	20 W	7 W			11½ W	14½ W	16 W
45 N	25 W	7½ W			11½ W	14 W	16 W
45 N	30 W	8 W			11¾ W	13¾ W	15¾ W
45 N	35 W	8¾ W			11¾ W	13½ W	15¾ W
45 N	40 W	9½ W			12 W	13 W	15 W
45 N	45 W	10½ W			12¾ W	13¾ W	15½ W
45 N	50 W	11½ W			13½ W	14 W	16 W
45 N	55 W	12½ W			14 W	15½ W	17 W
45 N	60 W	13¾ W			14 W	16 W	18½ W
50 N	5 W	7½ W				17 W	19¾ W
50 N	10 W	7¾ W				17½ W	19½ W
50 N	15 W	8½ W				17¾ W	20 W
50 N	20 W	9 W				17¾ W	20½ W
50 N	25 W	9¾ W				17¾ W	21 W
5 S	0	4¾ W			10 W	13½ W	15 W
5 S	5 W	2½ W			8½ W	11½ W	13 W
5 S	10 W	1 W	3 W	4¾ W	6½ W	9½	10¾ W
5 S	15 W	0	1½ W	3 W	4½ W	6¾ W	8½ W
5 S	20 W	1½ E	0¾ E	0¾ W	2¾ W	4 W	6 W

VARIATION.

Lati- tude.	Longi- tude, From London.	Anno 1700.	Anno 1710.	Anno 1720.	Anno 1730.	Anno 1744.	Anno 1756.
Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.
5 S	25 W	2 $\frac{1}{4}$ E	1 $\frac{1}{4}$ E	0 $\frac{1}{4}$ E	0 $\frac{1}{4}$ W	2 W	3 W
5 S	30 W	3 $\frac{1}{4}$ E	2 $\frac{1}{4}$ E	1 $\frac{1}{4}$ E	1 E	0 $\frac{1}{4}$ E	0
5 S	35 W	4 $\frac{1}{4}$ E	4 E	3 $\frac{1}{2}$ E	3 $\frac{1}{4}$ E	2 $\frac{3}{4}$ E	2 $\frac{3}{4}$ E
5 S	5 E	6 W			12 $\frac{1}{2}$ W	15 W	16 W
5 S	10 E	7 $\frac{1}{2}$ W			14 $\frac{1}{4}$ W	16 $\frac{1}{2}$ W	17 W
5 S	40 E	18 W	17 $\frac{3}{4}$ W	17 $\frac{3}{4}$ W	17 $\frac{1}{2}$ W	17 $\frac{1}{2}$ W	17 W
5 S	45 E	18 $\frac{1}{2}$ W	18 $\frac{1}{4}$ W	18 W	17 $\frac{1}{2}$ W	17 W	16 W
5 S	50 E	18 $\frac{1}{2}$ W	17 $\frac{3}{4}$ W	17 W	16 $\frac{1}{4}$ W	15 $\frac{1}{2}$ W	12 $\frac{3}{4}$ W
5 S	55 E	17 $\frac{1}{2}$ W	16 $\frac{1}{2}$ W	15 $\frac{1}{2}$ W	14 $\frac{3}{4}$ W	13 W	9 $\frac{1}{4}$ W
5 S	60 E	16 $\frac{1}{4}$ W	14 $\frac{3}{4}$ W	13 $\frac{1}{4}$ W	12 W	10 W	6 $\frac{1}{2}$ W
5 S	65 E	14 $\frac{3}{4}$ W	12 $\frac{3}{4}$ W	10 $\frac{3}{4}$ W	9 W	7 W	4 $\frac{1}{2}$ W
5 S	70 E	13 W	11 W	9 W	6 $\frac{3}{4}$ W	4 $\frac{1}{2}$ W	3 W
5 S	75 E	11 W	9 W	7 W	5 W	2 $\frac{1}{4}$ W	1 W
5 S	80 E	9 W	7 W	5 W	3 W	0 $\frac{3}{4}$ W	0
5 S	85 E	7 W	5 $\frac{1}{2}$ W	3 $\frac{3}{4}$ W	2 $\frac{1}{2}$ W	0 $\frac{1}{2}$ W	0 $\frac{3}{4}$ E
5 S	90 E	5 W	4 $\frac{1}{2}$ W	2 $\frac{3}{4}$ W	2 W	1 $\frac{1}{4}$ W	0 $\frac{3}{4}$ E
5 S	95 E	3 $\frac{3}{4}$ W	3 $\frac{1}{2}$ W	1 $\frac{1}{2}$ W	1 $\frac{1}{2}$ W	2 W	0 $\frac{1}{2}$ W
5 S	100 E	3 W	2 $\frac{1}{2}$ W	1 $\frac{1}{2}$ W	1 W	2 $\frac{3}{4}$ W	1 $\frac{1}{2}$ W
10 S	0	3 $\frac{3}{4}$ W			9 $\frac{1}{2}$ W	12 $\frac{1}{2}$ W	14 $\frac{1}{4}$ W
10 S	5 W	2 $\frac{3}{4}$ W	4 $\frac{1}{4}$ W	6 $\frac{1}{4}$ W	8 $\frac{1}{4}$ W	10 $\frac{1}{2}$ W	12 $\frac{3}{4}$ W
10 S	10 W	0 $\frac{3}{4}$ W	2 $\frac{3}{4}$ W	4 $\frac{3}{4}$ W	6 $\frac{1}{2}$ W	8 $\frac{1}{4}$ W	10 $\frac{1}{4}$ W
10 S	15 W	$\frac{1}{2}$ E	1 W	2 $\frac{1}{2}$ W	4 W	5 $\frac{3}{4}$ W	7 $\frac{3}{4}$ W
10 S	20 W	1 $\frac{3}{4}$ E	0 $\frac{1}{2}$ E	0	1 $\frac{1}{2}$ W	3 W	4 $\frac{1}{2}$ W
10 S	25 W	3 E	2 $\frac{1}{2}$ E	1 $\frac{1}{4}$ E	0 $\frac{3}{4}$ E	0 $\frac{3}{4}$ W	2 W
10 S	30 W	4 E	3 $\frac{1}{2}$ E	3 E	2 $\frac{1}{2}$ E	1 $\frac{1}{4}$ E	1 E
10 S	35 W	5 $\frac{1}{4}$ E	5 E	4 $\frac{1}{4}$ E	4 $\frac{1}{2}$ W	4 E	3 $\frac{1}{2}$ E
10 S	5 E	5 $\frac{3}{4}$ W			12 W	14 $\frac{1}{2}$ W	15 $\frac{3}{4}$ W
10 S	10 E	7 $\frac{1}{2}$ W			14 W	16 W	16 $\frac{3}{4}$ W

V A R I A T I O N.							
Latitude.	Longitude, From London	Anno 1700.	Anno 1710.	Anno 1720.	Anno 1730.	Anno 1744.	Anno 1756.
<i>Latitude.</i>	<i>Longitude.</i>	<i>Degrees.</i>	<i>Degrees.</i>	<i>Degrees.</i>	<i>Degrees.</i>	<i>Degrees.</i>	<i>Degrees.</i>
10 S	15 E	9 $\frac{1}{4}$ W			15 $\frac{1}{2}$ W	17 $\frac{1}{4}$ W	17 $\frac{3}{4}$ W
10 S	40 E	18 $\frac{3}{4}$ W	18 $\frac{3}{4}$ W	18 $\frac{3}{4}$ W	18 $\frac{3}{4}$ W	19 W	19 $\frac{1}{4}$ W
10 S	45 E	19 $\frac{1}{2}$ W	19 $\frac{1}{4}$ W	19 W	18 $\frac{3}{4}$ W	18 $\frac{1}{2}$ W	18 W
10 S	50 E	19 $\frac{1}{2}$ W	19 W	18 $\frac{1}{2}$ W	17 $\frac{3}{4}$ W	16 $\frac{3}{4}$ W	14 $\frac{1}{4}$ W
10 S	55 E	18 $\frac{1}{2}$ W	17 $\frac{1}{2}$ W	16 $\frac{1}{2}$ W	15 $\frac{1}{2}$ W	14 $\frac{1}{2}$ W	10 $\frac{1}{2}$ W
10 S	60 E	17 W	16 W	15 W	14 W	11 $\frac{1}{4}$ W	7 $\frac{1}{4}$ W
10 S	65 E	15 $\frac{3}{4}$ W	13 $\frac{3}{4}$ W	11 $\frac{3}{4}$ W	10 W	8 W	5 W
10 S	70 E	14 $\frac{1}{2}$ W	12 W	10 W	8 W	5 $\frac{1}{2}$ W	3 $\frac{1}{2}$ W
10 S	75 E	12 $\frac{1}{2}$ W	10 $\frac{1}{4}$ W	9 W	5 $\frac{3}{4}$ W	3 $\frac{1}{2}$ W	2 W
10 S	80 E	10 $\frac{1}{2}$ W	8 W	5 $\frac{1}{2}$ W	4 W	1 $\frac{3}{4}$ W	1 W
10 S	85 E	8 $\frac{1}{2}$ W	6 $\frac{3}{4}$ W	5 W	3 $\frac{3}{4}$ W	1 $\frac{1}{2}$ W	0 $\frac{1}{4}$ W
10 S	90 E	6 $\frac{3}{4}$ W	5 $\frac{1}{2}$ W	4 $\frac{1}{4}$ W	3 W	1 $\frac{3}{4}$ W	0 $\frac{1}{4}$ W
10 S	95 E	5 W	4 $\frac{1}{2}$ W	4 W	3 $\frac{1}{4}$ W	2 $\frac{1}{2}$ W	1 W
10 S	100 E	3 $\frac{3}{4}$ W	3 $\frac{1}{2}$ W	3 $\frac{1}{4}$ W	3 W	2 $\frac{3}{4}$ W	2 W
10 S	105 E	2 $\frac{3}{4}$ W	2 $\frac{3}{4}$ W	2 $\frac{1}{2}$ W	2 $\frac{1}{2}$ W	3 $\frac{1}{4}$ W	2 $\frac{3}{4}$ W
10 S	110 E	2 W				3 $\frac{3}{4}$ W	3 $\frac{1}{4}$ W
15 S	0	3 $\frac{1}{2}$ W	5 $\frac{1}{2}$ W	7 $\frac{1}{2}$ W	9 $\frac{1}{2}$ W	11 $\frac{1}{4}$ W	14 W
15 S	5 W	1 $\frac{3}{4}$ W	3 $\frac{3}{4}$ W	5 $\frac{3}{4}$ W	7 $\frac{3}{4}$ W	9 $\frac{1}{2}$ W	12 W
15 S	10 W	$\frac{1}{4}$ W	2 W	3 $\frac{3}{4}$ W	5 $\frac{1}{2}$ W	7 $\frac{1}{2}$ W	9 $\frac{1}{2}$ W
15 S	15 W	1 $\frac{1}{4}$ E	0 $\frac{1}{4}$ W	1 $\frac{1}{4}$ W	3 $\frac{1}{4}$ W	4 $\frac{1}{4}$ W	7 W
15 S	20 W	2 $\frac{1}{2}$ E	1 $\frac{1}{2}$ E	0 $\frac{3}{4}$ E	0 $\frac{1}{2}$ W	1 $\frac{1}{2}$ W	4 W
15 S	25 W	3 $\frac{1}{4}$ E	3 E	2 $\frac{1}{4}$ E	1 $\frac{1}{2}$ E	0 $\frac{1}{2}$ E	1 W
15 S	30 W	5 E	4 $\frac{1}{4}$ E	4 E	3 $\frac{1}{2}$ E	3 E	2 E
15 S	35 W	6 $\frac{1}{2}$ E	6 $\frac{1}{4}$ E	5 $\frac{3}{4}$ E	5 $\frac{1}{2}$ E	5 E	4 $\frac{1}{2}$ E
15 S	40 W	7 $\frac{1}{4}$ E				6 $\frac{1}{4}$ E	6 $\frac{1}{2}$ E
15 S	5 E	5 $\frac{1}{2}$ W			11 $\frac{1}{2}$ W	13 $\frac{1}{4}$ W	15 $\frac{1}{4}$ W
15 S	10 E	7 $\frac{1}{2}$ W			14 W	15 $\frac{1}{2}$ W	16 $\frac{1}{2}$ W
15 S	40 E	19 $\frac{3}{4}$ W	19 $\frac{1}{4}$ W	20 W	20 W	20 W	20 W

V A R I A T I O N.

Lati- tude.	Longi- tude, From London.	Anno 1700.	Anno 1710.	Anno 1720.	Anno 1730.	Anno 1744.	Anno 1756.
Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.
15 S	45 E	20 $\frac{1}{2}$ W	20 $\frac{1}{2}$ W	20 $\frac{1}{4}$ W	20 $\frac{1}{4}$ W	20 W	19 $\frac{1}{2}$ W
15 S	50 E	20 $\frac{1}{2}$ W	20 W	19 $\frac{1}{2}$ W	18 $\frac{3}{4}$ W	18 W	16 $\frac{1}{4}$ W
15 S	55 E	19 $\frac{1}{2}$ W	18 $\frac{1}{2}$ W	17 $\frac{1}{2}$ W	16 $\frac{1}{2}$ W	15 $\frac{1}{2}$ W	12 $\frac{1}{2}$ W
15 S	60 E	18 $\frac{3}{4}$ W	17 W	16 W	14 $\frac{1}{2}$ W	12 $\frac{1}{2}$ W	9 W
15 S	65 E	17 W	15 $\frac{1}{2}$ W	13 $\frac{1}{2}$ W	12 W	9 $\frac{3}{4}$ W	6 W
15 S	70 E	15 $\frac{1}{2}$ W	12 $\frac{3}{4}$ W	10 $\frac{3}{4}$ W	9 W	7 $\frac{1}{4}$ W	4 $\frac{1}{4}$ W
15 S	75 E	14 W	12 W	9 $\frac{3}{4}$ W	7 $\frac{1}{2}$ W	5 W	3 $\frac{1}{4}$ W
15 S	80 E	12 W	10 W	8 W	6 W	3 $\frac{1}{2}$ W	2 $\frac{1}{2}$ W
15 S	85 E	10 W	8 W	6 $\frac{3}{4}$ W	4 $\frac{3}{4}$ W	2 $\frac{3}{4}$ W	2 $\frac{1}{4}$ W
15 S	90 E	8 $\frac{1}{2}$ W	7 $\frac{1}{4}$ W	6 W	4 $\frac{1}{2}$ W	3 W	2 $\frac{1}{4}$ W
15 S	95 E	6 $\frac{1}{2}$ W	5 $\frac{1}{4}$ W	5 W	4 $\frac{1}{4}$ W	3 $\frac{1}{4}$ W	2 $\frac{3}{4}$ W
15 S	100 E	5 W	4 $\frac{3}{4}$ W	4 $\frac{1}{2}$ W	4 $\frac{1}{2}$ W	3 $\frac{3}{4}$ W	3 $\frac{1}{2}$ W
15 S	105 E	3 $\frac{1}{2}$ W	3 $\frac{1}{2}$ W	3 $\frac{3}{4}$ W	3 $\frac{3}{4}$ W	4 $\frac{1}{4}$ W	3 $\frac{3}{4}$ W
15 S	110 E	2 $\frac{1}{2}$ W				4 $\frac{1}{2}$ W	
20 S	0	3 $\frac{1}{4}$ W	5 $\frac{1}{4}$ W	7 $\frac{1}{4}$ W	9 W	11 W	13 $\frac{1}{2}$ W
20 S	5 W	1 $\frac{1}{2}$ W	3 $\frac{1}{4}$ W	5 W	6 $\frac{3}{4}$ W	8 $\frac{3}{4}$ W	11 $\frac{1}{4}$ W
20 S	10 W	0 $\frac{1}{2}$ E	1 $\frac{1}{4}$ W	3 W	4 $\frac{3}{4}$ W	6 $\frac{1}{2}$ W	8 $\frac{3}{4}$ W
20 S	15 W	1 $\frac{3}{4}$ E	0 $\frac{1}{2}$ E	0 $\frac{3}{4}$ W	2 W	3 $\frac{1}{2}$ W	5 $\frac{1}{4}$ W
20 S	20 W	3 E	2 $\frac{3}{4}$ E	1 $\frac{1}{2}$ E	0 $\frac{1}{2}$ W	0 $\frac{1}{2}$ W	3 W
20 S	25 W	4 $\frac{3}{4}$ E	4 $\frac{1}{4}$ E	3 $\frac{1}{2}$ E	2 $\frac{1}{4}$ E	2 E	0
20 S	30 W	6 E	5 $\frac{1}{4}$ E	5 $\frac{1}{4}$ E	4 $\frac{1}{4}$ E	4 $\frac{1}{4}$ E	2 $\frac{1}{2}$ E
20 S	35 W	7 $\frac{3}{4}$ E	7 $\frac{1}{2}$ E	7 $\frac{1}{4}$ E	6 $\frac{1}{4}$ E	6 $\frac{1}{2}$ E	5 E
20 S	40 W	9 $\frac{1}{4}$ E				8 E	7 $\frac{1}{2}$ E
20 S	5 E	5 $\frac{1}{2}$ W			11 W	13 $\frac{1}{4}$ W	15 W
20 S	10 E	7 $\frac{1}{2}$ W			13 $\frac{1}{4}$ W	15 W	16 $\frac{1}{4}$ W
20 S	15 E	9 $\frac{1}{2}$ W			15 $\frac{1}{2}$ W	16 $\frac{1}{2}$ W	17 $\frac{1}{2}$ W
20 S	35 E	19 W	19 $\frac{1}{4}$ W	19 $\frac{3}{4}$ W	20 $\frac{1}{4}$ W	20 $\frac{1}{2}$ W	22 W
20 S	40 E	20 $\frac{1}{2}$ W	20 $\frac{3}{4}$ W	21 $\frac{1}{4}$ W	21 $\frac{1}{2}$ W	21 $\frac{1}{2}$ W	22 W

V A R I A T I O N.							
Latitude.	Longitude, From London.	Anno 1700.	Anno 1710.	Anno 1720.	Anno 1730.	Anno 1744.	Anno 1756.
Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.
20 S	45 E	21 $\frac{1}{4}$ W	21 $\frac{1}{4}$ W	21 $\frac{1}{2}$ W	21 $\frac{1}{2}$ W	21 $\frac{1}{2}$ W	21 $\frac{1}{4}$ W
20 S	50 E	21 $\frac{1}{4}$ W	21 W	20 $\frac{3}{4}$ W	20 $\frac{1}{4}$ W	19 $\frac{3}{4}$ W	18 $\frac{3}{4}$ W
20 S	55 E	20 $\frac{1}{2}$ W	20 W	19 $\frac{1}{4}$ W	18 $\frac{1}{2}$ W	17 W	15 W
20 S	60 E	19 $\frac{1}{2}$ W	18 $\frac{1}{4}$ W	17 W	5 $\frac{3}{4}$ W	14 $\frac{1}{2}$ W	11 $\frac{1}{4}$ W
20 S	65 E	18 $\frac{1}{4}$ W	17 W	15 $\frac{3}{4}$ W	14 $\frac{1}{4}$ W	12 W	8 W
20 S	70 E	16 $\frac{3}{4}$ W	15 $\frac{1}{2}$ W	13 $\frac{1}{2}$ W	12 W	10 W	6 W
20 S	75 E	15 W	13 W	10 $\frac{1}{2}$ W	9 W	7 $\frac{1}{2}$ W	4 $\frac{3}{4}$ W
20 S	80 E	13 $\frac{1}{2}$ W	11 $\frac{1}{4}$ W	9 $\frac{1}{4}$ W	8 W	6 W	4 $\frac{1}{2}$ W
20 S	85 E	11 $\frac{1}{2}$ W	10 W	8 $\frac{1}{2}$ W	7 W	5 W	4 $\frac{1}{4}$ W
20 S	90 E	10 W	8 $\frac{3}{4}$ W	7 $\frac{1}{2}$ W	6 $\frac{1}{4}$ W	4 $\frac{1}{2}$ W	4 $\frac{1}{2}$ W
20 S	95 E	8 W	7 $\frac{1}{4}$ W	6 $\frac{1}{2}$ W	5 $\frac{3}{4}$ W	4 $\frac{1}{4}$ W	4 $\frac{1}{2}$ W
20 S	100 E	6 $\frac{1}{2}$ W	6 $\frac{1}{4}$ W	6 W	5 $\frac{1}{2}$ W	5 W	4 $\frac{3}{4}$ W
20 S	105 E	4 $\frac{1}{4}$ W	5 W	5 W	5 W	5 W	4 $\frac{1}{4}$ W
25 S	0	3 W	5 W	7 W	8 $\frac{1}{4}$ W	10 $\frac{1}{2}$ W	12 $\frac{1}{2}$ W
25 S	5 W	1 W	2 $\frac{1}{4}$ W	4 $\frac{1}{2}$ W	6 $\frac{1}{4}$ W	8 W	10 W
25 S	10 W	1 E	0 $\frac{1}{2}$ W	2 $\frac{1}{4}$ W	4 W	5 $\frac{1}{2}$ W	7 $\frac{1}{2}$ W
25 S	15 W	2 $\frac{1}{2}$ E	1 $\frac{1}{4}$ E	0	1 $\frac{1}{4}$ W	2 $\frac{1}{2}$ W	4 $\frac{1}{2}$ W
25 S	20 W	4 E	3 $\frac{1}{4}$ E	2 E	1 $\frac{1}{4}$ E	0 $\frac{1}{4}$ E	2 $\frac{1}{4}$ W
25 S	25 W	6 E	5 $\frac{1}{4}$ E	4 $\frac{1}{4}$ E	3 $\frac{1}{4}$ E	3 E	1 E
25 S	30 W	7 $\frac{1}{2}$ E	7 E	6 $\frac{1}{2}$ E	6 E	5 $\frac{1}{2}$ E	3 $\frac{1}{2}$ E
25 S	35 W	9 $\frac{1}{4}$ E				7 $\frac{1}{2}$ E	6 E
25 S	40 W	11 E				9 E	
25 S	5 E	5 $\frac{1}{4}$ W	7 W	8 $\frac{1}{4}$ W	10 $\frac{1}{4}$ W	12 $\frac{1}{4}$ W	14 $\frac{1}{4}$ W
25 S	10 E	7 $\frac{1}{2}$ W				14 $\frac{1}{4}$ W	16 W
25 S	15 E	9 $\frac{1}{2}$ W				16 $\frac{1}{4}$ W	17 $\frac{1}{4}$ W
25 S	35 E	19 $\frac{1}{4}$ W	20 $\frac{1}{4}$ W	20 $\frac{1}{4}$ W	21 $\frac{1}{2}$ W	22 $\frac{1}{4}$ W	23 $\frac{1}{4}$ W
25 S	40 E	21 W	21 $\frac{1}{4}$ W	22 W	22 $\frac{1}{2}$ W	23 $\frac{1}{4}$ W	23 $\frac{1}{2}$ W
25 S	45 E	22 $\frac{1}{4}$ W	22 $\frac{1}{2}$ W	22 $\frac{3}{4}$ W	23 W	23 $\frac{1}{2}$ W	23 W
25 S	50 E	22 $\frac{1}{2}$ W	22 $\frac{1}{2}$ W	22 $\frac{1}{2}$ W	22 $\frac{1}{2}$ W	22 W	21 W

V A R I A T I O N.

Lati- tude.	Longi- tude, From London.	Anno 1700.	Anno 1710.	Anno 1720.	Anno 1730.	Anno 1744.	Anno 1756.
Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.
25 S	55 E	22 W	21 $\frac{1}{2}$ W	21 W	20 $\frac{1}{2}$ W	19 $\frac{3}{4}$ W	18 W
25 S	60 E	20 $\frac{3}{4}$ W	19 $\frac{3}{4}$ W	19 W	18 $\frac{1}{4}$ W	17 W	14 $\frac{1}{2}$ W
25 S	65 E	19 $\frac{1}{4}$ W	18 $\frac{1}{4}$ W	17 $\frac{1}{4}$ W	16 $\frac{1}{4}$ W	15 W	11 $\frac{3}{4}$ W
25 S	70 E	17 $\frac{3}{4}$ W	16 $\frac{3}{4}$ W	15 $\frac{1}{2}$ W	14 $\frac{1}{2}$ W	13 W	9 $\frac{1}{2}$ W
25 S	75 E	16 $\frac{1}{4}$ W	15 W	13 $\frac{3}{4}$ W	12 $\frac{1}{2}$ W	11 $\frac{1}{4}$ W	8 W
25 S	80 E	14 $\frac{3}{4}$ W	13 $\frac{1}{2}$ W	12 $\frac{1}{4}$ W	10 $\frac{3}{4}$ W	9 $\frac{1}{4}$ W	7 $\frac{1}{2}$ W
25 S	85 E	13 W	11 $\frac{3}{4}$ W	10 $\frac{1}{2}$ W	9 $\frac{1}{4}$ W	8 W	7 $\frac{1}{4}$ W
25 S	90 E	11 $\frac{1}{4}$ W	10 $\frac{1}{4}$ W	9 $\frac{1}{4}$ W	8 $\frac{1}{4}$ W	7 $\frac{1}{4}$ W	7 W
25 S	95 E	9 $\frac{1}{2}$ W	9 W	8 $\frac{1}{2}$ W	7 $\frac{3}{4}$ W	7 W	6 $\frac{1}{2}$ W
25 S	100 E	7 $\frac{3}{4}$ W	7 $\frac{1}{2}$ W	7 $\frac{1}{4}$ W	7 W	6 $\frac{3}{4}$ W	6 W
30 S	0	2 $\frac{3}{4}$ W	4 $\frac{1}{4}$ W	6 W	7 $\frac{3}{4}$ W	9 $\frac{1}{2}$ W	11 $\frac{1}{4}$ W
30 S	5 W	0 $\frac{1}{2}$ W	2 W	3 $\frac{1}{2}$ W	5 $\frac{1}{4}$ W	7 W	9 W
30 S	10 W	1 $\frac{1}{2}$ E	0	1 $\frac{1}{2}$ W	3 W	4 $\frac{1}{2}$ W	6 $\frac{1}{2}$ W
30 S	15 W	3 $\frac{1}{4}$ E	2 $\frac{1}{4}$ E	0 $\frac{3}{4}$ E	0 $\frac{1}{2}$ W	1 $\frac{1}{2}$ W	3 $\frac{1}{2}$ W
30 S	20 W	5 E	4 $\frac{3}{4}$ E	3 $\frac{3}{4}$ E	2 $\frac{3}{4}$ E	1 $\frac{1}{2}$ E	0 $\frac{1}{2}$ W
30 S	25 W	7 $\frac{1}{4}$ E	6 $\frac{1}{2}$ E	5 $\frac{3}{4}$ E	5 E	4 E	2 E
30 S	30 W	9 E				6 $\frac{1}{4}$ E	4 $\frac{1}{2}$ E
30 S	35 W	11 E				8 $\frac{1}{4}$ E	7 $\frac{1}{4}$ E
30 S	5 E	5 W	7 W	8 $\frac{1}{2}$ W	10 $\frac{1}{4}$ W	11 $\frac{1}{4}$ W	13 $\frac{1}{4}$ W
30 S	10 E	7 $\frac{1}{4}$ W	9 $\frac{1}{4}$ W	11 $\frac{1}{4}$ W	12 $\frac{3}{4}$ W	14 W	15 $\frac{1}{2}$ W
30 S	15 E	9 $\frac{1}{2}$ W				16 W	18 W
30 S	30 E	17 $\frac{1}{2}$ W	18 $\frac{1}{2}$ W	19 $\frac{1}{2}$ W	20 $\frac{1}{2}$ W	21 $\frac{3}{4}$ W	23 $\frac{1}{4}$ W
30 S	35 E	20 $\frac{1}{4}$ W	21 W	21 $\frac{3}{4}$ W	22 $\frac{1}{2}$ W	23 $\frac{1}{2}$ W	24 $\frac{1}{4}$ W
30 S	40 E	21 $\frac{1}{2}$ W	22 $\frac{1}{2}$ W	23 $\frac{1}{4}$ W	24 W	24 $\frac{3}{4}$ W	25 $\frac{1}{4}$ W
30 S	45 E	23 W	23 $\frac{1}{2}$ W	24 W	24 $\frac{1}{2}$ W	25 $\frac{1}{4}$ W	24 $\frac{1}{2}$ W
30 S	50 E	23 $\frac{1}{2}$ W	23 $\frac{1}{4}$ W	24 W	24 $\frac{1}{4}$ W	24 $\frac{1}{2}$ W	23 $\frac{1}{4}$ W
30 S	55 E	23 W	23 W	23 W	22 $\frac{3}{4}$ W	22 $\frac{1}{2}$ W	21 W
30 S	60 E	21 $\frac{3}{4}$ W	21 $\frac{1}{2}$ W	21 W	20 $\frac{1}{4}$ W	20 W	18 W
30 S	65 E	20 $\frac{1}{4}$ W	19 $\frac{1}{4}$ W	19 W	18 $\frac{1}{2}$ W	17 $\frac{3}{4}$ W	15 W

V A R I A T I O ' N .

Latitude.	Longitude, From London.	Anno 1700.	Anno 1710.	Anno 1720.	Anno 1730.	Anno 1744.	Anno 1756.
Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.
30 S	70 E	18 $\frac{3}{4}$ W	18 W	17 $\frac{3}{4}$ W	16 $\frac{1}{2}$ W	15 $\frac{1}{2}$ W	13 W
30 S	75 E	17 $\frac{1}{4}$ W	16 $\frac{1}{2}$ W	15 $\frac{3}{4}$ W	14 $\frac{3}{4}$ W	13 $\frac{3}{4}$ W	11 $\frac{1}{2}$ W
30 S	80 E	15 $\frac{1}{2}$ W	15 W	14 W	13 W	12 W	10 $\frac{1}{4}$ W
30 S	85 E	14 W	13 $\frac{1}{4}$ W	12 $\frac{1}{2}$ W	11 $\frac{1}{2}$ W	10 $\frac{1}{2}$ W	10 $\frac{1}{4}$ W
30 S	90 E	12 $\frac{1}{2}$ W	11 $\frac{1}{4}$ W	11 W	10 $\frac{1}{4}$ W	9 $\frac{1}{2}$ W	9 $\frac{1}{2}$ W
30 S	95 E	10 $\frac{1}{2}$ W	10 W	9 $\frac{1}{2}$ W	9 $\frac{3}{4}$ W	8 $\frac{3}{4}$ W	9 W
30 S	100 E	8 $\frac{3}{4}$ W				8 $\frac{1}{4}$ W	
35 S	0	2 $\frac{1}{2}$ W	4 W	5 $\frac{1}{2}$ W	7 W	8 $\frac{1}{2}$ W	10 $\frac{1}{4}$ W
35 S	5 W	0	1 W	2 $\frac{1}{2}$ W	4 W	5 $\frac{3}{4}$ W	7 $\frac{1}{4}$ W
35 S	10 W	2 $\frac{1}{4}$ E	1 $\frac{1}{2}$ E	0 $\frac{1}{2}$ W	1 $\frac{1}{4}$ W	3 $\frac{1}{4}$ W	5 $\frac{1}{2}$ W
35 S	15 W	4 $\frac{1}{4}$ E				0 $\frac{1}{4}$ W	2 $\frac{1}{4}$ W
35 S	20 W	6 $\frac{3}{4}$ E				2 $\frac{1}{2}$ E	0 $\frac{1}{2}$ E
35 S	25 W	8 $\frac{3}{4}$ E				5 E	3 E
35 S	30 W	10 $\frac{1}{4}$ E				7 $\frac{1}{4}$ E	5 $\frac{3}{4}$ E
35 S	35 W	12 $\frac{1}{4}$ E				9 $\frac{1}{4}$ E	8 $\frac{1}{4}$ E
35 S	5 E	5 W	6 $\frac{1}{2}$ W	8 W	9 $\frac{1}{2}$ W	11 W	13 W
35 S	10 E	7 $\frac{1}{4}$ W	8 $\frac{1}{2}$ W	10 $\frac{1}{4}$ W	11 $\frac{3}{4}$ W	13 $\frac{1}{2}$ W	15 $\frac{1}{4}$ W
35 S	15 E	9 $\frac{3}{4}$ W	11 $\frac{1}{4}$ W	12 $\frac{3}{4}$ W	14 $\frac{1}{4}$ W	15 $\frac{1}{2}$ W	17 $\frac{1}{2}$ W
35 S	20 E	12 $\frac{1}{2}$ W	14 W	15 $\frac{1}{2}$ W	17 W	18 $\frac{1}{2}$ W	19 $\frac{1}{2}$ W
35 S	25 E	15 $\frac{1}{4}$ W	16 $\frac{1}{2}$ W	18 W	19 $\frac{1}{4}$ W	20 $\frac{1}{2}$ W	22 $\frac{1}{2}$ W
35 S	30 E	18 $\frac{1}{4}$ W	19 $\frac{1}{2}$ W	20 $\frac{1}{2}$ W	21 $\frac{1}{2}$ W	22 $\frac{1}{2}$ W	24 $\frac{1}{4}$ W
35 S	35 E	21 W	22 W	22 $\frac{1}{4}$ W	23 $\frac{1}{2}$ W	24 $\frac{1}{2}$ W	26 W
35 S	40 E	22 $\frac{3}{4}$ W	23 $\frac{1}{2}$ W	24 $\frac{1}{4}$ W	25 W	26 W	26 $\frac{1}{2}$ W
35 S	45 E	24 $\frac{1}{4}$ W	25 W	25 W	26 W	27 W	26 $\frac{1}{2}$ W
35 S	50 E	24 $\frac{3}{4}$ W	25 $\frac{1}{4}$ W	25 $\frac{3}{4}$ W	26 W	26 $\frac{1}{4}$ W	24 $\frac{3}{4}$ W
35 S	55 E	24 $\frac{1}{2}$ W	24 $\frac{1}{2}$ W	24 $\frac{1}{2}$ W	24 $\frac{3}{4}$ W	25 W	23 W
35 S	60 E	23 W	23 W	23 W	22 $\frac{1}{2}$ W	22 $\frac{1}{4}$ W	21 W
35 S	65 E	21 $\frac{1}{2}$ W	21 $\frac{1}{4}$ W	21 W	20 $\frac{3}{4}$ W	20 $\frac{1}{4}$ W	18 $\frac{3}{4}$ W
35 S	70 E	19 $\frac{3}{4}$ W	19 $\frac{1}{4}$ W	18 $\frac{3}{4}$ W	18 $\frac{1}{4}$ W	17 $\frac{3}{4}$ W	16 $\frac{1}{4}$ W

V A R I A T I O N.

Lati- tude.	Longi- tude, From London.	Anno 1700.	Anno 1710.	Anno 1720.	Anno 1730.	Anno 1744.	Anno 1756.
Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.
35 S	75 E	18 $\frac{1}{4}$ W	17 $\frac{3}{4}$ W	17 $\frac{1}{4}$ W	17 $\frac{1}{2}$ W	16 W	15 $\frac{1}{4}$ W
35 S	80 E	16 $\frac{3}{4}$ W	16 $\frac{1}{4}$ W	15 $\frac{3}{4}$ W	15 W	14 $\frac{1}{4}$ W	14 $\frac{1}{4}$ W
35 S	85 E	15 W	14 $\frac{1}{2}$ W	14 W	13 $\frac{1}{2}$ W	13 W	13 $\frac{1}{2}$ W
35 S	90 E	13 $\frac{1}{2}$ W	13 W	12 $\frac{3}{4}$ W	12 $\frac{1}{2}$ W	12 W	12 $\frac{1}{4}$ W
35 S	95 E	11 $\frac{1}{2}$ W	11 $\frac{1}{2}$ W	11 $\frac{1}{4}$ W	11 W	10 $\frac{3}{4}$ W	
40 S	0	2 W	3 $\frac{1}{2}$ W	5 W	6 $\frac{1}{2}$ W	7 $\frac{3}{4}$ W	9 $\frac{1}{4}$ W
40 S	5 W	0 $\frac{3}{4}$ E	0 $\frac{1}{2}$ W	2 W	3 $\frac{1}{2}$ W	5 W	6 $\frac{3}{4}$ W
40 S	10 W	3 $\frac{1}{4}$ E				2 $\frac{1}{2}$ W	4 $\frac{1}{4}$ W
40 S	15 W	5 $\frac{1}{2}$ E				0 $\frac{1}{2}$ E	1 $\frac{1}{2}$ W
40 S	20 W	8 E				3 $\frac{1}{4}$ E	1 $\frac{1}{4}$ E
40 S	25 W	10 $\frac{1}{2}$ E				5 $\frac{1}{2}$ E	4 E
40 S	30 W	12 $\frac{1}{2}$ E				8 E	6 $\frac{1}{2}$ E
40 S	5 E	4 $\frac{1}{2}$ W	6 W	7 $\frac{1}{2}$ W	9 W	10 $\frac{1}{4}$ W	12 W
40 S	10 E	7 $\frac{1}{4}$ W	8 $\frac{1}{2}$ W	10 W	11 $\frac{1}{2}$ W	12 $\frac{3}{4}$ W	14 $\frac{1}{2}$ W
40 S	15 E	9 $\frac{3}{4}$ W	11 $\frac{1}{4}$ W	12 $\frac{3}{4}$ W	14 W	15 $\frac{1}{2}$ W	17 $\frac{1}{4}$ W
40 S	20 E	12 $\frac{3}{4}$ W	14 $\frac{1}{4}$ W	15 $\frac{1}{2}$ W	17 W	18 $\frac{1}{2}$ W	20 W
40 S	25 E	16 W	17 $\frac{1}{4}$ W	18 $\frac{1}{2}$ W	19 $\frac{3}{4}$ W	21 $\frac{1}{4}$ W	22 $\frac{3}{4}$ W
40 S	30 E	19 W	20 W	21 $\frac{1}{4}$ W	22 $\frac{1}{4}$ W	23 $\frac{1}{2}$ W	25 $\frac{1}{4}$ W
40 S	35 E	21 $\frac{3}{4}$ W	22 $\frac{3}{4}$ W	23 $\frac{1}{2}$ W	24 $\frac{1}{2}$ W	25 $\frac{1}{2}$ W	27 W
40 S	40 E	23 $\frac{1}{4}$ W	24 $\frac{1}{2}$ W	25 W	26 W	27 W	28 $\frac{1}{2}$ W
40 S	45 E	25 $\frac{3}{4}$ W	26 W	26 $\frac{3}{4}$ W	27 $\frac{1}{2}$ W	28 $\frac{1}{4}$ W	27 $\frac{3}{4}$ W
40 S	50 E	26 W	26 $\frac{1}{2}$ W	27 W	27 $\frac{1}{2}$ W	28 $\frac{1}{4}$ W	26 W
40 S	55 E	25 $\frac{1}{4}$ W	25 $\frac{1}{2}$ W	25 $\frac{3}{4}$ W	26 W	26 $\frac{1}{2}$ W	24 $\frac{1}{2}$ W
40 S	60 E	24 W	24 W	24 W	24 $\frac{1}{4}$ W	24 $\frac{1}{4}$ W	22 $\frac{3}{4}$ W
40 S	65 E	22 $\frac{1}{2}$ W	22 $\frac{1}{2}$ W	22 $\frac{1}{4}$ W	22 $\frac{1}{4}$ W	22 W	20 $\frac{3}{4}$ W
40 S	70 E	20 $\frac{3}{4}$ W	20 $\frac{1}{2}$ W	20 $\frac{1}{4}$ W	20 W	19 $\frac{1}{2}$ W	19 $\frac{1}{2}$ W
40 S	75 E	19 $\frac{1}{4}$ W	18 $\frac{3}{4}$ W	18 $\frac{1}{4}$ W	17 $\frac{3}{4}$ W	17 $\frac{1}{4}$ W	18 $\frac{1}{4}$ W
40 S	80 E	17 $\frac{1}{2}$ W	17 W	16 $\frac{1}{2}$ W	16 W	15 $\frac{1}{2}$ W	17 $\frac{1}{4}$ W

VARIATION of the Magnetic-Needle, from the Islands of Orkney to Hudson's Straits, for the Year 1757.

West Longitude From London.		Degrees of North Latitude.							
Degrees.		56	57	58	59	60	61	62	63
4	Degrees of West Variation.				18	18	19	19	
10				19	19	20	20	21	
27				24	24	25	25		
45		29	29	30	31				
55									
65						39	40	41	

VARIATION in Hudson's-Bay and Straits, for the Year 1757.

West Longitude From London.		Degrees of North Latitude.									
Degrees.		52	55	56	57	58	59	60	61	62	63
65	Degrees of West Variation.							39	40	41	
71									41	41	
79											43
81									38	39	40
83		18	20							39	40
86									35	37	
92					17	17					
94						17	18				
95							18				

We have been informed, that in Hudson's-Bay, there has been very little alteration in the variation of the compass during the twenty years last past.

XLII. *An Account of some extraordinary Tumors upon the Head of a labouring Man, now in St. Bartholomew's Hospital.*
By James Parsons, M. D. F. R. S.

Read Nov. 10, 1757. **T**HIS poor man, whose name is John Tomlinson, gives this account of himself: That he was born at or near Rotherham in Yorkshire, and is now about 25 years of age: that when he was a boy of four or five years old, at play with other children, he received a blow from one of them upon the top of his head; and believes that hurt, he then received, was the beginning of the appearances, that are represented before you. See TAB. XIV. The tumor upon the top of his head, however, grew first, and, after having spread all over the vertex, extended gradually downwards over his right shoulder, and forwards over the ~~frontis~~ ^{frontis}, on the same side, till it stretched downwards into a lax flabby substance all over the right side of his face and shoulder: then the upper of the three anterior tumors arose from the large one; the middle one from the *ala nasi*, pulling it down by its weight, as you see it in Figure 1.; and the lower one was pendulous from the inside of the great tumor by a narrow neck. These are the appearances which present themselves at first sight; but those under the great tumor are no less extraordinary; for, upon lifting up the great tumor, and looking up

* The two figures shew a fore and back view of this subject.
4 under



Parsons MD. ad viv. del.



under it, his right eye comes in sight, with which he sees very well, and the eye is clear and sound; but the under lid is pulled down, and stretched to six or seven inches long, to which a tumor hung also, as large as that anterior one at the chin, the lowest of the three; besides several flaps and *rugæ* of skin, and smaller tumors.

The hairy scalp is so stretched by the vertical tumor, that the hairs are driven asunder; so that the tumor is in some places bald, and the whole is rugged and uneven. At its basis, all round, till we come to the extended part that goes away to the right shoulder, a bony edge may be distinctly felt, as if the skull was depressed at the top: and yet I cannot but believe, that there is no depression of the arch of the inner table, because the man was from his childhood ever very healthy; being never troubled with those symptoms, which usually attend a depression of the *cranium*. From this seeming edge the *os frontis* shoots out a great way over the *os nasi*, perhaps to two or three inches beyond the frontal sinus's; and is the basis, from which the great pendulous tumor hangs downwards and forwards.

From the root of the nose, under the upper of the three smaller tumors, arises a large trunk of a vein, which ramifies up to the vertical tumor, and up the right over the upper part of the great pendulous one: these are very conspicuous, and serve to bring back the residual blood from the tumors: nor is it unlikely that the arteries bear a proportion with these veins in their size, in order to supply the tumors with the matter, which has given them their great increase; but these, lying concealed, cannot be spoken to with any certainty.

If we compare this growth of the frontal bone with that of other *exostoses*, I believe there may this difference be rationally observed; that other *exostoses* are generally attended with ulcerous tumors, which are for the most part cancerous; and these may commence at any age. I have now drawings, taken from the right hand of a man of 50, which represent risings of the *radius* and *ulna*, with the fingers, to a most frightful degree; and these begun but six years before, and are attended with foul running ulcers; and now the bones of the arm and hand, on the left side, are beginning to have the same appearances: whereas the frontal bone of the present subject appears sound, as far as we are able to judge by examination: nor does there appear the least disposition to ulceration in any part of it. When this is the case, the growth generally begins while the subjects are young; upon which we shall be more particular a little further on. His sensation upon every part of these tumors, is exactly like that of every other part of his skin, having not the least uneasiness upon being handled. This poor man worked at day-labour in the fields till some months before he came to town.

Perhaps it may not be improper to lay down the dimensions of these tumors, as the case is so extraordinary; for the size of them is almost incredible: but I made my drawing in the presence of several of the gentlemen of that hospital, who allowed it to be very exact, and precise in the expression of the parts, as well as in the dimensions. The vertical tumor is about seven inches diameter at the basis, where the bony edge is felt, mentioned before, and about four inches high from that edge. From that edge,

edge, or the basis of the vertical tumor, to the bottom of the great tumor, is ten inches ; so that the length of both, from the vertex to the end of the great one, is about 14 inches : and upon viewing it, when he turns his side towards you, the whole mass is eight or nine inches over all the way ; hard at top, and flabby downwards, hanging in kinds of plaits. From the eye to the opposite outline of the great tumor is six inches ; and lower down, from the left corner of his mouth to the opposite outline of the same tumor, eight inches. The upper small tumor, over the nose, is one inch three quarters long by one inch and a half ; the middle tumor is two inches long from the *ala nasi*, to which it hangs, and of the same breadth ; and the lowest tumor, shaped like a goose's egg, is four inches and a half long by near three inches over.

This man is under the care of Mr. Crane, an eminent surgeon of St. Bartholomew's Hospital, who has just now taken off the lowest of these three anterior tumors, and also the tumor mentioned, which hung underneath to the under lid of his right eye. He intends proceeding to take off that at the *ala nasi* next, and so on till he takes away all the smaller tumors first : afterwards the larger will be considered. The substance of those cut off was intirely fat ; nor was there the least speck of blood in the lowest of the three smaller tumors ; but there was an hæmorrhage from a vessel divided in taking off that hanging to the right eye-lid ; which soon yielded to the methods he made use of, and went on, successfully till quite healed.

It is pity no one of the people of condition in the
 VOL. 50. Z z country,

country, where this poor man lived, took notice of him while he was a lad ; because, when the vertical tumor begun, or even after it had made some progress, if he had been sent up to any of our hospitals, there would have been no difficulty in curing him. This leads me to some precautions, which, I hope, will render my account of the case of some use, considered in a physiological light ; which was my intention in thus laying it before this learned Society.

There is a great deal of difference between injuries received in young subjects and in adults. In the latter, the consequences are not apt to be of so dangerous a nature as in the former (except indeed where there happen violent fractures or wounds, which immediately dispatch the person, young or old) ; because, in such as are so far advanced in years, as that the parts have done growing, or, in other words, are incapable of carrying the person to any larger size, preserving the natural proportion, a tumor arising from a blow on the head would be merely local, without extending to any neighbouring parts in so extraordinary a manner : but in children, as in the case before you, a tumor may increase every moment from a blow, and spread itself to the neighbouring parts, to the ruin of the child, unless timely care be taken to prevent it ; because in such young subjects the parts are continually growing, the vessels enlarging in their diameters, and carrying more and more nutrition to every point, in proportion to the nature of each individual organ, always preserving such an equilibrium, in the distribution of the nutritive juices, as is proper to secure the due proportion of every part as it increases : but when a tumor arises
from

from a blow in such a growing subject, if no wound is made, nor suppuration brought on in the tumor, then the parts of the tumor being only weakened, the equilibrium is destroyed, a greater flux of juices than ordinary is carried to it, the due resistance being impaired, and a luxuriance of growth is produced in the place of the injury, which greatly exceeds that of the rest of the body; and will most certainly continue in the same manner, during the growth of the subject, when once thus begun. In the present subject, this luxuriance was communicated even to the veins, which are apparent and large, and which were before, in their natural state, scarce visible; and not only to these, but to the very bones of the forehead: and as to the integuments and membranes of the body, their great distensibility is well known to every one. I have seen an *ovarium* so distended by water, and thickened as it grew, that it had substance enough to bear being dressed by a tanner, and contained nine gallons, which I saw poured into it after it was dressed. And does not every corpulent person shew the same power of distension in the membranes and integuments of the body, as well as wens of all kinds upon the surface?

I thought so extraordinary a case well worth the notice of the learned members of this Society in itself; and the more so, as these few hints fall naturally from it, to render its publication useful. We are taught by this, how necessary it is for all such as have the management of youth under their care, to have an early regard to every accident that may befall children; for many times injuries of this kind have been thought very trivial, which, being overlooked

and neglected too long, have been followed by very direful circumstances. I have known a young gentleman to have great hard swellings about his head, and become epileptic, losing his senses as he advanced in years, from a blow with the back of a book given him by a master. I am, with due respect,

The SOCIETY'S

Sept. 18. 1757.

Most obedient Servant,

James Parsons.

XLIII. *An Extract of the Register of the Parish of Great Shefford, near Lamborne, in Berkshire, for Ten Years: With Observations on the same: In a Letter to Tho. Birch, D. D. Secret. R. S. from the Rev. Mr. Richard Forster, Rector of Great Shefford.*

Great Shefford, near Lamborne,
Berks, July 8. 1757.

Rev. Sir,

Read Nov. 17,
1757.

WHEN I settled in the country, abundant leisure enabled me to keep an exact parish-register. I have now finished ten years, I trust, with sufficient care, having examined every thing accurately myself. The sight of three letters, lately published in the Transactions, upon the subject of political arithmetic, put me upon

over-

overlooking and methodizing my own account; which I here send you, to make what use you think proper of it.

From Lady-day 1747. to D°. 1757.

Baptized -	{ Males — 73 }	{ Females — 75 }	— — — 148
Buried --	{ Males — 44 }	{ Females — 39 }	— — — 83
			Increase — 65

Buried {	Under 2 years of age	— 25
	Between 2 & 5	— 4
	5 — 10	— 3
	10 — 20	— 4
	20 — 30	— 5
	30 — 40	— 9
	40 — 50	— 4
	50 — 60	— 4
	60 — 70	— 9
	70 — 80	— 11
	80 — 87	— 5
		<u>83</u>

And but one alive above 87, who is 91.

The Number of People 425.

The Number of Houses 90.

The Number of Acres 2245. whereof $\frac{1}{8}$ is waste.

I do not offer such trifling numbers as these, as a fit subject to build a canon of life upon; but only

as they may furnish us with a few particulars, which may throw some small light upon a subject hitherto very little cultivated : and as what has been advanced this way has been always taken from great cities, a little from the country perhaps may not be disagreeable.

The first observable in my numbers is, that the two infancies of human life are exactly equal ; i. e. as many die above 60 as under 2 years of age ; and that these two periods of life are by much the most sickly, five eighths of the whole, nearly, dying in these two stages, which renders the intermediate numbers very small.

This will give us some reason to suspect, that capital cities are very improper to estimate the probabilities of life from. The continual flux of people from the circumjacent country, to seek for employment, makes the decrements of life seem much larger than they really are. London is very remarkable upon this account ; and Breslaw must receive pretty large accessions, as a very considerable manufacture is carried on there.

The second thing I would observe from my table is, that it confirms what Dr. Brackenridge observes of the Isle of Wight ; *viz.* that the births are to the burials as 2 to 1 almost ; ours being as 15 to 8 nearly. Now if this is the case of all the country places in England, it will give us a strong presumption, that the increase of mankind is much quicker than Dr. Derham's proportion of 1 to 12 ; especially if we consider,

Thirdly, That of the living not 1 in 50 dies yearly ; and this in a village not very healthy. We are situated upon the celebrated Lamborne stream, which

which dries up generally in August, and leaves a stagnated water, and stinking mud, at a critical season of the year, which bring on a putrid fever, and make our place sometimes very sickly. In the year 1751 we buried 17, and in 1756. 11: and therefore we may presume, that in the healthiest parts of the nation, the proportion is still greater, perhaps not one in 60. In order to clear up this, it were to be wished, that the actual number of the people was known, where-ever the bills of mortality are exhibited. All reasoning without this preliminary is really not much better than groping in the dark.

A fourth thing observable from my numbers is, that the quantity of people allotted to a house is too big in all former calculations: for if we divide 425, the number of people, by 90, the number of houses, it gives but 4.72, which is not quite $4\frac{3}{4}$ to a house; and therefore 5 to a house, I believe, is as much as ought to be allowed, taking the nation all together. Now if the number of houses, taken in Queen Anne's time, be any thing near the right, with one fourth more allowed for cottages, according to Dr. Brackenridge's computation, we shall make the people in England, allowing 5 to a house, to be only 4,556,550. which appears, at first sight, to be too small a number. However, of Shefford I would beg leave to observe, (and it is far from being the poorest of villages) that more than two thirds of all the houses are downright cottages, and must be excluded, one as much as another, from any proposed assessment. Upon this foundation we must grant, that at least half the houses in England, take towns and all together, must be cottages, and plead an exemption from taxation
all

all alike. And thus the number of houses will be 1,458,096. which, multiplied by 5, will give us the number of people, 7,290,480. If to this we add the proposed increase, 789,558. we shall have 8,080,038 for the number of people now alive in England.

The fifth and last thing I would observe from my numbers is, that we may hence guess at the number of people in the whole kingdom: for if 1871, the good acres in Shefford, demand 425 persons for their cultivation, then will 25,300,000 good acres in England require 5,704,168 for the cultivation of the land only. Now supposing one third part of the people only to live in towns, above what is necessary for the cultivation of the land belonging to such towns, then we must add 2,852,084 to the above sum, which gives us 8,556,252 for the number of people in England. It may probably here be said, that this is but little better than reckoning at random. Indeed I allow it is so. But then I must beg leave to observe, that it has full as good a foundation to stand upon, as any calculation, that I have seen hitherto advanced. It has one *datum*, viz. a certain number of persons to a certain number of acres. It ought to be noted at the same time, that we are an inland place, have no sort of manufacture carried on, and consequently no accession of strangers.

If we examine the calculation arising from the consumption of wheat, we shall see some reason to suspect, that the number of inhabitants in England is not short of eight millions. I am persuaded I do not exaggerate, when I affirm, that three fourths of the people north of Trent, and in Wales, do not eat

eat wheat : and as this is near a third part of England, it will follow, that one fourth of the whole is left out of the calculation, and that we must add near two millions to it to make it complete.

Again, I compute, that in my parish there are killed annually 160 fat hogs, *viz.* above one to three persons ; and that this humour of pig-killing prevails over half of England at least, and is in some measure indulged in all parts. Now we will suppose, that there are but six millions of people in the nation, and that what is killed in the northern half makes up for what is deficient by reason of towns in the southern half ; we must from hence conclude, that a million of fat hogs are killed in England every year. Now one hog with another takes two quarters of corn, sometimes barley, sometimes pease : if we put half barley, we shall be under the truth. And here we shall have a million quarters of barley, not only to balance the exportation of wheat, but also to be equivalent to, as much bread-corn as will maintain a full million of people.

Farther, it is well known, that the greatest part of the corn-trade is, of late years, got into the hands of millers : and it has been whispered about for a considerable time, and, I think, now the millers do not deny it, that *some* whitening is carried to all the great mills. The excuse alleged for it is, that it makes the flour *wet*, and consequently *bake*, the better. I am rather inclined to be of opinion, that it is to give a colour to something that wants colour. And indeed, whoever tastes the common bakers bread against a piece of genuine wheat-bread, will have some reason to suspect, that all is not gold, that glitters. Every body

knows, that the millers buy large quantities of barley and pease, they say, to fat hogs : but then they have pollard, middlings, &c. to fat them with ; and so may possibly mix the barley and pease with wheat to grind. But as this is all surmise, I would have no more weight laid upon it than it deserves.

The next article is of the same nature ; I mean, something of a mystery in trade ; and therefore to be touched very gently. What I would hint is, that it is the opinion of many very intelligent persons, that a good deal of malt is made, which does not pay the excise. I do not pretend to ascertain the quantity : perhaps one eighth may not be an extravagant supposition. And if this be the case, we shall find as much barley, as will weigh against bread for half a million of people.

But here, in all probability, you will object, that if all these articles be admitted, we shall make the number of people near eleven millions ; which is undoubtedly too much. I am ready to grant it. And here, if I might take the liberty to speak my mind, I think, that the allowance of one quarter of wheat to three persons is too scanty, and must quite starve the poor, whose chief provision is bread : and therefore, two persons to a quarter may be pretty near the truth. And then the numbers will stand thus :

Such as eat wheat, by supposition -	4,500,000
In the North, and in Wales - -	1,500,000
Against the fattening article - - -	1,000,000
Against the two last articles - -	1,000,000

8,000,000

I can-

I cannot conclude this long scroll without recommending it strongly to the members of the Royal Society, who have many of them seats in parliament, and most of them interest in those that have, to get an Act passed for perfecting registers. The trouble is trifling; the expence nothing. It would be of great service likewise to number the people: and this might be done with great ease. I was not three hours in finishing mine on foot; tho' it is, perhaps, as extensive, for the number of people, as most in England, being near five miles in length. I am,

Reverend Sir,
Your affectionate Brother,
and very humble Servant,
Richard Forster, *Rector*.

XLIV. *A remarkable Case of an Aneurism, or Disease of the principal Artery of the Thigh, occasioned by a Fall. To which is prefixed a short Account of the Uncertainty of the distinguishing Symptoms of this Disease. By Jos. Warner, F.R.S. and Surgeon to Guy's Hospital.*

Read Nov. 17,
1757.

WHEN the coats of an artery become by any means præternaturally distended, when they become wounded, or when they become ruptured in such a manner as to

discharge,

A a a 2

discharge and deposit their former contents under the neighbouring integuments, under the aponeurosis, or tendinous expansion of a neighbouring muscle, or still more deeply under the muscles themselves; the natural consequence attending this accident will sooner or later be a degree of elevation, or tumor: which species of tumor is known by the term *aneurism*.

If a true aneurism happens, that is, a swelling arising from a general weakness of the coats of an arterial vessel, or from a wound or rupture of some of its coats, it may be often distinguished from a tumor proceeding from any other cause by a degree of pulsation, supposing the situation of the injured vessel be superficial; as may be evinced in recent aneurisms of the humeral artery, which sometimes happen from bleeding near the bending of the elbow-joint; as well as in aneurisms of the inferior part of the radial artery, of the ulnar artery, or of the anterior artery of the leg called *tibialis antica*; and as may be observed to be sometimes the case too in those arteries, whose situations are not superficial; to wit, in aneurisms of the *aorta ascendens*, the curvature of the *aorta*, and of the *carotides*.

The symptom of pulsation in tumors, which take their rise from a partial wound, or from a general weakness, and subsequent dilatation of the coats of an artery, is not confined to this species of aneurism, but is frequently attendant upon false aneurisms (that is, such tumors, as are occasioned by extravasated arterial blood), supposing the disease to be a recent one of either of the preceding vessels, or of any other arterial vessel not deeply situated: and this

this symptom of pulsation in false aneurisms will sometimes be accompanied with a discoloration, or variegated appearance, of the integuments dependent upon the insinuation of the blood underneath them.

But if the extravasation be confined under an aponeurosis, or if the disease has been of so long standing, as to admit of the thinner parts of the extravasated blood being absorbed, or by any other means dispersed, and the fibrous parts, which are left behind, should be accumulated in considerable quantities, and acquire so compact and solid an appearance, as to resemble brown macerated leather in their colour and texture, which I have always observed to be the case in old diseases of this kind; under these circumstances, the original symptoms of pulsation on the swelling, and a discoloration of the integuments, for the most part become imperceptible: for which reasons the true nature of the disease must be attended with a degree of uncertainty.

It must be acknowledged by all those, whose experience has given them opportunities of examining into these diseases, that the symptoms of a pulsation, and a discoloration of the teguments from extravasated blood, are not only very often wanting in old aneurisms, but in the most recent ones: which proves the non-existence of these symptoms to be no certain characteristics of tumors not being aneurismal: and the reason why this often happens may be readily explained, and conceived of, from demonstrating the very deep or low situation of many arteries, that are known to be liable to these injuries; such as the femoral arteries, the *arteriæ tibiales posticæ*, the *arteriæ peroneæ*, and some others.

Notwith-

Notwithstanding I have treated of pulsation on tumors, and a discoloration of the integuments or coverings of the part, when they do exist, as being the truest marks of aneurisms; yet it must not be inferred from what has hitherto been advanced, that the appearances of these symptoms are unexceptionable rules of tumors being aneurismal; seeing it does happen, that mere imposthumations, or collections of matter, arising from external as well as from internal causes, are sometimes so immediately situated upon the heart itself, and at other times upon some of its principal arteries, as to partake in the most regular manner of their contraction and dilatation (systole and diastole).

Some years ago I saw an instance of a boy, about 13 years of age, who had his breast-bone much broken by a fall. On this account he was admitted into Guy's Hospital; but not till a fortnight after the accident happened.

Upon examination, there appeared an evident separation of the broken parts of the bone, which were removed at a considerable distance from each other: the intermediate space was occupied by a tumor of a considerable size: the integuments were of their natural complexion: the tumor had as regular a contraction and dilatation as the heart itself, or the aorta could be supposed to have.

Upon pressure, the tumor receded; upon a removal of the pressure, the tumor immediately resumed its former size and shape. All these are the distinguishing signs of a true recent aneurism. The situation and symptoms of this swelling were judged sufficient reasons for considering the nature of the disease

discase as uncertain; on which account it was left to take its own course. The event was, the tumor burst in three weeks after his admittance, discharged a considerable quantity of matter, and the patient did well.

From what has been above advanced it is plain, if these arguments can be supported by facts, that the laying down such rules for infallibly distinguishing aneurismal tumors from tumors proceeding from very different causes, must be a matter of the greatest difficulty: and, as a further proof of their uncertainty, I take the liberty of offering the following short history of a remarkable case, which has lately occurred in my own experience.

In the month of December 1756. John Yates, aged 35 years, received an hurt upon and about his knee, by falling upon the ground from a man's back. The accident was immediately followed with a considerable degree of lameness and pain; which upon standing or walking were greatly increased.

He continued in much the same state for about six weeks after the accident. At the end of this time, the calf or the leg was attacked with an cedematous or doughy swelling; which, in a fortnight, became so painful, as to disable him from walking. The tumor continued to increase for about eight weeks; and at length extended itself so far upwards, as to affect the greatest part of the thigh, the whole of which was attended with excessive pain, but more particularly so about the knee.

N. B. So far I relate from the patient's own account.

On the 28th of April 1757. he was admitted into Guy's Hospital under my care.

Upon examination, the thigh appeared enlarged to a very great size. The tumor was uniform, and extended from the inside of the knee to within a very small space of the groin. The integuments were in every part of their natural colour.

Upon pressing the tumor on the inside, it appeared soft, and there was a very evident fluctuation to be felt on its internal and lateral part; but there was not the least appearance of pulsation.

The tumor, on its superior and posterior parts, was of a stony hardness.

The leg, which, according to the patient's account, had some time ago been much swelled, did not now appear to be at all so.

He was continually in great pain, and had been for some time incapable of getting any sleep. His appetite was bad. He was a good deal emaciated. He had a constant slow fever, which arose about five weeks before his admission into the hospital. He appeared pale and fallow in his complexion.

From the time of his being placed under my care to the end of ten days, there was no alteration in the swelling, or in the symptoms attending it.

In expectation therefore of affording him that relief, which could by no other means be procured, I judged it adviseable to make an opening into the tumor; which I did by incision into the most prominent and fluctuating part; upon which there immediately gushed out a large stream of thin florid blood, and at this instant discovered to me the true state of that disease; which, till now, could not be ascertained

tained by any peculiar symptom distinguishable by the touch, or perceptible to the eye.

Seeing this, I immediately filled up the wound with lint and tow ; and then proceeded, in as expeditious a manner as possible, to apply a tight bandage upon the thigh, near to the groin ; and, lest this might accidentally break, I applied a second ligature below the first, and proceeded to amputate the limb upon the spot.

During the operation the man fainted, but soon recovered from this deliquium ; and, without any bad symptoms, gradually recovered his rest, appetite, and strength, and is now in perfect health.

Upon a dissection of the thigh and leg, I discovered the following appearances :

A great part of the fleshy portions of two of the extensor muscles of the leg, to wit, the *vastus internus*, and *crureus*, were destroyed, with the subjacent *periosteum*.

Four of the muscles, whose uses are to bend the leg, and which compose the internal and external hamstrings ; to wit, *gracilis*, *semitendinosus*, *semimembranosus*, and *biceps tibiæ*, together with that adductor and flexor muscle of the leg called *sartorius*, were removed at a considerable distance from the thigh-bone on its inferior part, and from the *tibia* and *fibula* on their superior parts ; by which means a large bed or cavity was formed for containing the extravasation, which consisted partly of a fluid, and partly of a coagulated blood ; but by far the greatest part of the coagulum had acquired so firm and fibrous a consistence and appearance, as nearly to resemble brown macerated leather in its colour and

texture. The neighbouring muscles appeared livid and lacerated.

The *os femoris* was become carious on its inferior and posterior parts; and, at about an inch distance above the condyle of that bone internally, there arose a considerable *exostosis*.

The capsular ligament of the knee-joint was become much thickened, and contained about two ounces of a viscid yellow *synovia*.

The femoral artery, on its inferior part, just above its division into *tibialis antica* and *postica*, was diseased; which disease extended four inches upwards.

The coats of the artery were considerably thickened, and lacerated longitudinally.

The smallest diameter of the diseased part of the artery was two inches and one quarter: the largest diameter of the diseased part of the artery was two inches and one half.

Hatton-Garden,
Nov. 17. 1757.

XLV. *Farther Experiments for increasing the Quantity of Steam in a Fire-Engine.* By Keane Fitz-Gerald, Esq; F. R. S.

Read Nov. 24, 1757. **I** Gave a former account to the Royal Society of some experiments made for increasing the quantity of steam in a fire-engine, by blowing air thro' boiling water *. The effects

* See above, N^o. X. p. 53.

then evidently produced left me, and I believe many others, who came to view the experiments, no room to doubt the seeming cause. In which error I should probably have still remained, had not farther experiments demonstrated the mistake.

Whatever apology I ought to make this learned Society, for having given in that account prematurely, I believe their great regard to truth, which has always been the basis of their researches for the improvement of natural knowledge, will require none for this. I shall therefore, as briefly as I can, relate the further experiments, that were made, which evidently demonstrate the error of the former; and from which some phenomena have occurred, perhaps hitherto unknown.

In order to try what difference the air passing thro' a thinner body of water might occasion, I brought the horizontal pipe, which (as mentioned in the former account) was placed 12 inches under the surface of the water, to within six inches; and found, on setting the engine to work, that the leaden pipe, for the conveyance of air from the bellows into the boiler, became much hotter than I had perceived it before; which could not happen, if a constant cool air had passed thro': and on shutting the cock, which was fixed in the leaden pipe to hinder the steam from ascending into the bellows before the engine should be set to work, tho' no air could then possibly pass thro', yet the bellows still continued to move with the same regularity as before; which, on examination, was found defective on the inside, where the middle board, that divides the two bodies, was warped and cracked in several places, thro' which

the air passed very regularly from one body to the other at each stroke, instead of passing thro' the pipe into the boiler, as imagined. By this, the cause of deception was evident; which I was still in hopes of remedying, by having a new pair of bellows made, somewhat larger, and much stronger. When this was fixed, and the engine worked a few strokes, I was surprised to find the bellows did not come down, but remained fully charged with air, tho' it had 400 lb. weight upon it; and that, on increasing the weight gradually to 1400 lb. which was as much as the bellows could support, the air was not forced thro'.

I also made several experiments, by lowering the horizontal pipe two feet under the surface of the water, and raising it at different times to within four inches of the surface, and could not at any depth force the air thro', whilst the engine worked; but on opening the steam-pipe, which is a pipe for letting the steam pass from the boiler whenever the engine stops, the bellows could then readily force the air thro', tho' the water boiled ever so strong, and seemingly made a surprising increase of steam.

I had the leaden pipe to convey the air from the bellows, which was first put thro' the top into the boiler, carried on the outside, and passed horizontally into it, about the height the water generally stands, that by opening a cock, fixed for the purpose close to the boiler, I could readily discharge all the steam lodged in the pipe; and by shutting the cock, and making small holes at three or four inches distance, I could almost find the point, where the air and steam met in opposition, cool air being strongly expelled thro' one, and hot steam thro' the other.

It

It was also perceptible, that the air was impelled somewhat, tho' not considerably, more forward by the addition of each hundred weight on the bellows :

That the deeper the horizontal pipe was placed in the water, the less resistance was made by the steam :

That in proportion as the heat of the steam was increased, by making the water boil more strongly, the resistance to the pressure of the air by the weight on the bellows became greater.

It is a very doubtful matter, whether air forced thro' boiling water would have answered the purpose intended : but I believe it was never imagined, that air could not be readily forced thro', until proved by the foregoing experiments. The attempt, tho' it has failed demonstrably in that point, has produced the same effect from another cause, as to saving coals, and throwing up more water. For, by the constant care, that was taken during the time of making these experiments, to measure the coals, to admit only a proper quantity of fuel to be laid on, and also to mark the time exactly it took in burning; the engine then did, and still continues to require eight bushels of coals less, in every 24 hours work, than it did before; and also, from the regularity of its stroke, to throw up more water; the same care being required from the engineer, who can have no pretence for consuming more coals now, than appeared sufficient during the time the experiments were making.

Tho' some of the properties of steam are well known; yet the degrees of expansion it is capable of,

of; whether air be mixed with, or necessary to, its formation; as also how far its power of resistance may reach; are probably not yet known, to a proper degree of exactness. Niewentit fixes the expansion of a cubical inch of water, converted into steam, at 13365, Dr. Desaguliers at 14000, and Mr. Payne at 4000 times. The great scope in this subject from a plenum to a vacuum, if I may be allowed the expression, as also the very useful purposes, to which it has already been, and possibly may be still further applied, will, I hope, be an inducement to those, who are much better qualified, to proceed in so useful an inquiry.

XLVI. Observatio Eclipsis Lunæ Die 27 Martii, Ann. 1755. habita Ulissipone in Domo Patrum Congregationis Oratorii à Joanne Chevalier ejusdem Congregationis Presbytero, Regiæ Londinensis Societatis Socio, Regiæque Parisiensis Scientiarum Academicæ correspondente.

Tubo optico 8 pedum perfecta est observatio cœlo sereno, claroque.

IMMERSIONES.

Hora postmeridiana
temporis veri.

		^h	[']	["]
Read Dec. 8, 1757.	I nitium penumb rædubium	10	29	50
	I nitium eclipsis dubium	10	33	35
Certe jam incæperat	— — —	10	34	05
Umbra ad mare humorum	— — —	10	44	00
			Umbra	

	"	'	"
Umbra ad Grimaldum — —	10	44	53
Grimaldus totus in umbra ———	10	47	58
Mare humorum totum in umbra —	10	51	14
Thico incipit mergi — — —	10	53	29
Thico totus mergitur — — —	10	55	14
Umbra ad Reinholdum ———	11	08	04
Umbram ingreditur Copernicus —	11	19	22
Umbra ad mare nectaris ———	11	24	52
Totum in umbra — — —	11	33	50
Umbra ad mare tranquillitatis —	11	35	24
Promontorium acutum in umbra —	11	45	46

EMERSIONES.

	"	'	"
Incipit emergere ab umbra Copernicus	12	04	38
Totus Copernicus extra umbram --	12	07	40
Incipit egredi Grimaldus ———	12	09	38
Totus Grimaldus extra umbram —	12	12	38
Incipit emergere mare fecunditatis	12	31	37
Emergit mare humorum ———	12	36	11
Incipit emergere Capuanus ———	12	39	40
Egreditur Schicardus --- —	12	48	30
Emergit Thico — — —	12	51	40
Totum mare nectaris egreditur —	12	58	09
Finis eclipsis — — —	13	13	02
Finis penumbrae dubius ———	13	16	50

XLVII. *Eclipsis Lunæ Die 4^a Februarii, Ann. 1757. habita Ulissipone à Joanne Chevalier Presbytero Congregationis Oratorii, Regiæ Londinensis Societatis Socio, Regiæque Scientiarum Parisiensis Academiæ correspondente, et a Theodoro de Almeida ejusdem Congregationis Presbytero, ac Physicæ publico Professore.*

Read Dec. 8.

1757.

HANC observationem peregi tubo optico novem pedes longo, cujus lens ocularis focum habebat ad 4 pollices, et lineam unam: adhibui preterea vitrum planum cæruleum, quod oculum inter et ocularem lentem interponebam, ut ingressum macularum in umbram observarem juxta ea quæ in observatione eclipsis lunæ ann. 1755. invenit clarissimus vir Josephus Soares de Barros ex regia Berolinensi academia. Primum igitur vitro cæruleo adhibito observabam ingressum maculæ in umbram, et tempore notato iterum solo tubo optico ingressum ejusdem maculæ in umbram observabam, et differentiam utriusque ingressus notabam.

Initio eclipsis cælum serenum ac clarum fuit, postea nubilum, et post maximam obscurationem vapores horizontis et claritas incipientis diei observationem peragere impedire.

				Manè.		
				H.	M.	s.
Initium penumbræ	—	—	4	52	49	
Initium dubium eclipsis	—	—	4	55	29	
Certo jam incæperat	—	—	4	57	30	
				Umbræ		

	Manè.		
	H.	M.	S.
Umbra ad Aristarchum observata vi-			
tro cæruleo plano — — —	5	00	19
Solo tubo optico adhibito — — —	5	00	50
Keplerus umbram ingreditur observa-			
tus tubo, et vitro cæruleo plano —	5	13	20
Observatus solo tubo optico — —	5	14	00
Plato umbram ingreditur observatus			
tubo, et vitro cæruleo — — —	5	15	2
— Solo tubo — — —	5	15	40
Umbra ad Eudoxum — — —	5	17	18
Mare serenitatis incipit mergi — —	5	30	10
Copernicus observatus tubo et vitro			
cæruleo umbram ingreditur — —	5	36	48
— Solo tubo — — —	5	37	22
Mare Crisium ingreditur umbram —	5	53	51
— Medium in umbra — — —	5	59	30
— Totum mergitur — — —	6	5	21
Mare fecunditatis occultari incipit —	6	7	41
Umbra ad promontorium acutum —	6	8	33
Umbra tangit mare nectaris ^{iv.} — —	6	22	51
Umbra ad Langrenum — — —	6	23	33

*Observationes Eclipsium Satellitum Jovis Ulissipone
habite a JOANNE CHEVALIER, &c.*

ANNO 1757 telescopio Gregoriano 7 pedum.
nocte serena, nullaque lunari luce illustrata, ob-
servavi immersionem totalem primi satellitis die 21
Martii tempore vero postmeridiano — 11^h 13' 1"

Die vero 22 Martii tempore vero, et antemeridiano,
observavi immersionem totalem tertii satellitis

0^h 13' 32"

XLVIII. *Observationes Eclipsium Satellitum Jovis Ulissipone habitæ à Joanne Chevalier, Presbytero Congregationis Oratorii, Regiæque Londinensis Societatis Socio, Anno 1757.*

Read Dec. 8. 1757. **T**elescopio Gregoriano 7 pedum observavi emerfionem primi satellitis die 7 Junii cum cœlum serenum ac clarum effet, hora postmeridiana temporis veri — $10^h 29' 21''$

Die 8 Junii eodem telescopio observavi emerfionem secundi satellitis hora postmeridiana $8^h 32' 48''$ cœlum aliquantum nubilum erat.

Eadem die observavi emerfionem tertiî satellitis hora postmeridiana — — — $9^h 36' 25''$ cœlo claro.

Die 15 Junii cœlo elaro observavi emerfionem secundi satellitis hora postmeridiana — $11^h 6' 15''$

Sequenti die 16 Junii observavi immerfionem tertiî satellitis hora matutina temporis veri — $0^h 0' 29''$

XLIX. *A remarkable Case of the Efficacy of the Bark in a Mortification. In a Letter to William Watson, M. D. F. R. S. from Mr. Richard Grindall, Surgeon to the London Hospital.*

Austin-Friars, Dec. 7th, 1757.

S I R,

Read Dec. 8, 1757. **T**HE following case being very singular has induced me to lay it before the Royal Society, and beg the favour to do it through your means. Although numerous instances are related in the records of medicine, of the great danger in interrupting nature in her operations, there is not one (so far as I know), in which more violent and extraordinary effects have been produced, than in the following.

It may happen also, that this instance may be of service in ascertaining the virtue of the medicine in intermittents, when in the hands of men of judgment.

On the 28th of June 1757. Mary Alexander, aged 31 years, of the parish of Whitechapel, was brought into the London hospital, having a mortification in both hands, which reached about an inch and half above the wrists. All her toes, and about an inch of one foot beyond the last joint, were mortified; her nose was also intirely destroyed by a mortification; and all these happened at the same time. Upon inquiry into the cause of this misfortune, I found, that on Monday the 30th of May she was seized with a quotidian

quotidian ague, which usually began about three of the clock in the afternoon, and lasted near two hours; which was succeeded by a hot fit, and then a violent sweat. And in this manner she was afflicted for seven days without any material alteration; when, being informed by a neighbour, of a person, who had an infallible remedy for the cure of an ague, she applied to him. He brought her two phials, containing about an ounce and half each, of a pale yellowish liquor; one of which he directed her to take directly, promising, that she should have no return of the fit of consequence; and that, if she had any small return, the second bottle should cure her effectually. In consequence of which, she took one dose, which was at the time the cold fit had been on about a quarter of an hour: she had no sooner swallowed it, but, as she says, her stomach was on fire, and felt as if she had swallowed the strongest dram possible. The cold fit left her instantly; but she was immediately seized with so violent a fever, as to make her burn, and be extremely thirsty, all the following night; much more so than ever she had been before, till the next morning, when a sweat a little relieved her from the violent heat. When she rose in the morning, she was much troubled with a great itching in the hands, feet, and nose; and soon after all those parts began to feel numbed, or, as she describes it, as if her hands and feet were asleep; which she took but little notice of, till the evening of that day, when she found the nails of both hands and feet were turning black, and, at the same time feeling great pain in both, as also in her nose, and that they appeared of a darkish red colour, like the skin in cold weather.

Upon

Upon which, at nine o'clock that night she sent for an apothecary, from whom, I have since been informed, the person before mentioned had bought the medicine, which he gave her. The apothecary was not at home; his journeyman went, and finding the woman had a difficulty of breathing, ordered her a mixture with *sperma ceti* and *ammoniacum* to be taken occasionally. The apothecary did not see her himself till the 16th of June, when finding her in a very bad condition, that her hands, and feet, and nose, were entirely black, and had many vesicles or small bladders upon them, filled with a blackish bloody water; he opened them, and let out the fluid, and dressed them with yellow basilicon; and in this manner continued treating her till the 20th of the same month, when, finding no material alteration for the better, he ordered her a brownish mixture, of which she was to take four spoonfuls every four hours; which, he informed me, was a decoction of the bark; and says, on taking this, she was better, as the mortification seemed inclined to stop. But as it was a bad case, he advised the woman to be carried to an hospital: and in this condition was she brought in, when she was immediately put into a course of the bark, taking a drachm of the powder every four hours; and in 48 hours taking it there was a perfect separation of all the mortified parts. She was then ordered to take it only three times in 24 hours; and pursuing this method for eight days, there was a very good digestion from the parts above the mortification.

The mortified part became now so offensive, that the poor woman pressed me much to take off her hands, assuring me she would go through the operations

tions

tions with good courage, being very desirous to live, though in this miserable condition.

On the 12th of July I took off both her hands: I had very little more to do, than saw the bones, nature having stopped the bleeding, when she stopped the mortification. In a day or two after, I took off all the toes from both feet, and now discontinued the bark, the parts appearing in a healthy and healing condition; which went on so for five weeks, when, on a sudden, the parts began to look livid, her stomach failed her, and she was feverish; but, upon taking an ounce of the bark, in 36 hours her sores began again to look well. She was not suffered to leave off the bark so soon this time, but continued taking it twice a day for a month. She is now almost well: that part of her face, from whence the nose mortified, was healed in seven weeks; the stumps of both arms are intirely healed; and both feet are well, only waiting for one piece of bone scaling off, which I believe will be in a very short time; and she is now in good health.

The person, who gave her this medicine, is a Barber and Peruke-maker at Bow. I applied to him several times, to inform me what it was he had given her. The affair was talked of so much in his neighbourhood, and the man threatned by the woman's husband, that for a long time I could not get him to tell me, till I told him, I had been informed where he bought the medicines; and the time of the day, that he had them, corresponding with the time of his giving them to the woman, and that I knew it was tincture of myrrh, he at last told me, that he had frequently given the above quantity of an ounce and half of

of it in an ague; that it had never done any harm; and hardly ever failed to cure. Upon which information, I carried some tincture of myrrh to the woman, who tasted it, and is well assured it is the same liquor the barber gave her in her ague-fit.

I am, with respect,

Your obliged and obedient Servant.

Richard Grindall.

*L. A Letter to the Rev. Tho. Birch, D.D.
Secret. R. S. from John Pringle, M.D.
F.R.S. inclosing Two Papers communicated
to him by Robert Whytt, M.D. F.R.S.*

S I R,

Pallmall-Court, St. James's,
Dec. 10. 1757.

Read Dec. 15.
1757.

ABOUT three weeks ago I put into your hands an extract of a letter, I had then received from Dr. Whytt, containing a postscript to his *Observations on Lord Walpole's Case*; and slightly mentioning some doubts he had then about the justness of Dr. Springsfeld's experiments with lime-water, from some trials he himself had made, upon reading that gentleman's curious treatise on the extraordinary lithontriptic quality of the waters at Carlsbad in Bohemia. Within these few Days, Dr. Whytt having favoured me with a full account of those experiments, I have herewith sent you his paper, in order, if you please, to lay it before

fore the Society; which the author desires may be done, in case these observations should be judged useful.

The other paper inclosed was sent me by the same hand, to be likewise presented to the Society, as a well-attested instance of the electrical power in the cure of a palsy. To the other testimonies I have subjoined what Dr. Whytt says in his letter to me, by way of strengthening the evidence. I shall only add, that since Mr. Brydone, the author of this account, has omitted telling how long the patient has continued in perfect health since the operation, it appears she must have been well for some months before the date of his paper; because, before the end of last summer, Dr. Whytt transmitted the same case to me, which I then returned, in order to have it drawn up in a fuller manner, and with other vouchers besides the gentleman, who performed the cure. The Doctor has been so good as to comply with my request, having procured a more ample account of the circumstances from Mr. Brydone, and the attestation of two ministers, besides that of the patient herself.* My difficulties being thus removed,

* After this paper was read at the Society, Dr. Pringle having acquainted Dr. Whytt, that Mr. Patrick Brydone had omitted, in his account, the name of the parish, where the woman lived, the time when she was cured, and also that he had not fully dated his paper; Dr. Whytt some time after wrote to Dr. Pringle, that having desired Mr. Brydone to furnish him with these particulars, he had received for answer, "That the woman, on whom the cure was performed, had lived all her life in the parish of Coldingham, and for the last twelve years in that town: That her father had
" died

moved, I believe I may now with freedom offer this very curious case to the attention of the Society.

I am,

S I R,

Your most obedient humble Servant,

John Pringle.

*Postscript to Dr. Whytt's Observations on Lord
Walpole's Case*.*

Read Dec. 8, 1757. “ **I** Do not know, if it be worth while
“ to observe, that lately, in making
“ some experiments with different *calculi*, there was
“ one almost as white as chalk, but of a less hard
“ substance than the others; and which was not
“ in the least degree dissolved or softened by being
“ infused 20 days in oystershell lime-water, but
“ yielded somewhat to a solution of Spanish soap
“ in common water.

“ From this experiment one may conclude, that
“ it is better to prescribe both soap and lime-water

“ died of the palsy seven years ago, after having been subject to
“ that distemper for several years: That the cure was performed
“ in his father's house at Coldinghame, on the 4th, 5th, 6th,
“ and 11th of days of April 1757. a circumstance he had noted
“ down: That as to the date of his paper, presented to the Royal
“ Society, he only recollects it was written some day in the be-
“ ginning of November last; but as the woman still continued
“ well, he hoped the precise day of the month was no material
“ omission.” This letter to Dr. Whytt is dated, Coldinghame,
January 9th, 1758.

* See above, p. 209, & seqq.

“ for the stone, than any one of them alone ; and
 “ that if one of these remedies has failed of giving
 “ relief, the other ought to be tried : for as the
 “ above white *calculus*, which yielded a little to the
 “ solution of soap, resisted lime-water ; so there may
 “ perhaps be others, that are readily dissolved by
 “ lime-water, but little affected by soap.

“ Dr. Springsfeld’s experiments with lime-water
 “ are somehow not just ; for in several *calculi* I
 “ have found the dissolving power of oystershell
 “ lime-water above eight times greater than he
 “ makes it.”

*Some Observations on the lithontriptic Virtue
 of the Carlsbad Waters, Lime-water, and
 Soap: In a Letter to Dr. John Pringle,
 F. R. S. from Dr. Robert Whytt, F. R. S.
 and Professor of Medicine in the University
 of Edinburgh.*

S I R,

Read Dec. 15.
 1757.

FROM the experiments related in Dr. Springsfeld’s *Commentatio de prerogativa thermarum Carolinarum, &c.* which you were so good as to send me some time ago, it appears, that these waters are not only possessed of a very extraordinary power of dissolving the stone, but that in this respect they greatly exceed lime-water.

(A) Thus, Dr. Springsfeld having infused, for 14 Days, in a heat of 96 degrees of Fahrenheit’s scale, three pieces of the same *calculus*, each weighing 30 grains,

grains, in eggshell lime-water, the Carlsbad water, and in the urine of one who daily drank this last water, renewing these several menstruums every day, he found, on the 15th day, that the *calculus* in the lime-water had lost 1 grain, the *calculus* in the Carlsbad water 6 grains, and that in urine 5 grains.

(B) Again, having divided another *calculus* into four parts, each of which was reduced to 80 grains, he put the first in oystershell lime-water, the second in Carlsbad water, and the third in the urine of a person who drank this water. After 20 days, during which time the menstruums were renewed every day, and kept in a heat of 96 degrees, the dried *calculi* had lost of their weight as follows: the first 3 grains, the second 18 grains, and the third 14 grains.

Altho' I make no doubt that Dr. Springsfeld, who appears to be a man of candour, as well as learning, has faithfully related the event of the experiments, which he made; yet either the lime-water he used must have been very weak, or some other mistake must have happened in his experiments: for in all the numerous trials I made, about 15 years ago, of lime-water, as a solvent for the stone, I always found its dissolving power much greater, than it appears in Dr. Springsfeld's experiments. And as in these trials different urinary stones were used, it can scarcely be imagined, that it was owing to the peculiar hardness of Dr. Springsfeld's *calculi*, that the lime-water made so little impression on them. However, to be still further satisfied of this matter, I made the following experiments.

1. I put a piece of a very hard *calculus*, which I shall call *x*, weighing 80 grains, in oystershell lime-water, renewing the lime-water every day, and keeping it in a heat between 90 and 106 degrees of Fahrenheit's scale. After 20 days, I took out the *calculus*; and having set it by for some days, till it was become quite dry, I brushed away all the rotten part of it, which was reduced to a kind of chalky powder, and found that the undissolved part of it weighed 57 grains.

2. At the same time a piece of another *calculus*, *z*, weighing 15 grains, was, after a like infusion of 20 days in oystershell lime-water, reduced to 10 grains.

3. I put a piece of *z*, weighing 14 grains, in a solution of half an ounce of the internal part of Spanish soap in nine ounces of water, and every third day renewed the solution, which was kept in a heat of about 60 degrees. After 14 days, I found the undissolved part not to exceed 11 grains.

4. A piece of a white chalky *calculus*, *y*, weighing 30 grains, had near 4 grains of its substance dissolved, by being 14 days infused as above in a solution of soap.

From N°. 1. above, compared with Dr. Springfield's Exper. (B), it appears, that the dissolving power of oystershell lime-water is to that of the Carlsbad water as 23 to 18, supposing the *calculi* used in these experiments to have been equally easy to dissolve.

N°. 3. compared with Dr. Springfield's Exper. (A), shews, that the dissolving power of a solution of the inner part of Spanish soap, in a heat of 60 degrees,
is

is to that of the Carlsbad water, in a heat of 96 degrees, as 15 to 14.

From N^o. 4. compared with (A), the dissolving power of soap is to that of the Carlsbad water only as 4 to 6; but it is probable, that had the solution of soap been kept in a heat of 96 degrees, its dissolving power would, even in this experiment, have nearly equalled that of the Carlsbad water. It may, perhaps, be worth while to observe, that a piece of the white chalky *calculus* of N^o 4. was not in the smallest degree dissolved by lying in lime-water 20 days.

5. In Exper. 19. of my Essay on the Virtue of Lime-water, a piece of a *calculus*, *b*, weighing 3 r grains, lost 7 grains by being infused 36 hours, in a heat of above 100 degrees, in very strong oyster-shell lime-water. And in the same water, of a moderate strength, another piece of *b* lost, in the same time, 5 grains.

In this last experiment, the lithontriptic virtue of lime-water appears to be stronger than in N^o. 1. and 2. above; and greatly exceeds that of the Carlsbad water in Dr. Springsfeld's Exper. (A) and (B).

But altho', from what has been said, it appears not only that lime-water, but also a solution of soap, dissolves the stone in close vessels as fast, nay faster, than the *thermæ Carolinæ*; yet these last waters, when the *calculi* were so placed in open vessels, that the water from the fountain might constantly flow along them, effected a much quicker dissolution than lime-water, or even soap-lye, or indeed any known menstruum, except, perhaps, strong spirit of nitre :

for, in the first experiment made by Dr. Springsfeld, a *calculus* of two ounces and a half was, in this manner, quite dissolved in six days. From this experiment, compared with that of Dr. Springsfeld mentioned above (B), it will be found, upon calculation, that the dissolving power of the Carlsbad water, when it is allowed to flow constantly from the fountain along the stone, is nearly 39 times greater than when it is only poured fresh on the *calculus* once a day *. What may have been the reason of this surprising difference of the lithontriptic power of the Carlsbad water in these different circumstances, I will not pretend to say. I think it can scarcely be accounted for from the gentle motion of the water along the surface of the *calculus*. Was it then owing to some very volatile active part, which the water quickly loses, after being taken from the fountain?

But how great soever the dissolving power of the Carlsbad waters may be, when they issue from the bowels of the earth, yet that they do not communicate a much greater dissolving power to the urine, than lime-water, will appear from comparing the two following experiments.

In Dr. Springsfeld's Exper. (A) above, the urine of a person, who drank the Carlsbad waters, reduced, in 14 days, a piece of *calculus*, weighing 30 grains, to 25 grains. And in an experiment made by Dr. Newcome, now Lord Bishop of Llandaff, who drank four English pints of oyster-shell lime-water

* Vid. Essay on the Virtue of Lime-water, 2d edit. p. 176, 177.
daily,

daily, his Lordship's urine reduced, in four months, a piece of *calculus*, weighing 31 grains, to three small bits, weighing in all 6 grains †. Whence it follows, that the dissolving power of his Lordship's urine must have been to the dissolving power of the urine of the person who drank the Carlsbad waters nearly as 35 to 65 ‡. But if we consider, that the *calculus* infused in the urine of the person who drank the Carlsbad waters was kept always in a heat of 96 degrees, while in Dr. Newcome's experiment, which was made during part of the autumn and winter, no artificial heat was used, it will appear probable, that the dissolving power of his Lordship's urine was little inferior to that of the person who drank the Carlsbad waters; for lime-water, in a heat of 96 degrees, dissolves the *calculus* at least twice as fast, as in the common heat of the air in winter. Further, if it be attended to, that the quantity of Carlsbad waters drank every day before dinner is from six to eight lib. while his Lordship only drank four lib. of lime-water in 24 hours, it will follow, that whatever the different dissolving powers of the lime-water and Carlsbad waters may be out of the body, yet the former seems, in proportion to the quantity drank, to communicate at least an equal dissolving power to the urine.

But without presuming to decide certainly, as to the comparative virtue of the Carlsbad waters and lime-water, I shall conclude with observing, that tho' the Carlsbad waters are less disagreeable to the

† Essay on Lime-water, 2d edit. p. 208, &c.

‡ Ibid. p. 176 and 177.

taste, and may be drank in larger quantity, than lime-water, yet this last may be drank equally good in all places, and at all seasons of the year; which is not the case with the Carlsbad waters.

November 30. 1757.

*An Instance of the Electrical Virtue in the
Cure of a Palsy. By Mr. Patrick Bry-
done.*

Read Dec. 15. 1757. **E**Lizabeth Foster, aged 33, in poor circumstances, unmarried, about 15 years ago was seized with a violent nervous fever, accompanied with an asthma, and was so ill, that her life was despaired of. She recovered however from the violence of her distemper, but the sad effects of it remained. For, from this time, she continued in a weakly uncertain state of health till the month of July, 1755, when she was again taken ill of the same kind of fever; and after it went off she was troubled with worse nervous symptoms than ever, ending at last in a paralytic disorder, which sometimes affected the arm, sometimes the leg, of the left side; in such a manner as that these parts, tho' deprived of all motion for the time, yet still retained their sensibility. In this condition she remained till the spring 1756, when unexpectedly she grew much better; but not so far as to get quite rid of her paralytic complaints; which, in cold weather, seldom failed to manifest themselves by a numbness, trembling, sensation of cold, and a loss of motion in the left side.

This

This paralytic tendency made her apprehensive of a more violent attack ; which accordingly soon happened : for, about the end of August, in the same year, her symptoms gradually increased, and in a very short time she lost all motion and sensation in her left side. In this state she continued throughout last winter with the addition of some new complaints ; for now her head shook constantly ; her tongue faltered so much, when she attempted to speak, that she could not articulate a word ; her left eye grew so dim, that she could not distinguish colours with it ; and she was often seized with such an universal coldness and insensibility, that those who saw her at such times scarce knew whether she was dead or alive.

Whilst the woman was in this miserable condition, observing that she had some intermissions, during which she could converse and use her right leg and arm, in one of those intervals I proposed trying to relieve her by the power of electricity. With this view, I got her supported in such a manner as to receive the shocks standing, holding the phial in her right hand, whilst the left was made to touch the gun-barrel. After receiving several very severe shocks, she found herself in better spirits than usual ; said she felt a heat, and a prickling pain, in her left thigh and leg, which gradually spread over all that side ; and after undergoing the operation for a few minutes longer, she cried out, with great joy, that she felt her foot on the ground.

The electrical machine producing such extraordinary effects, the action was continued ; and that day the woman patiently submitted to receive above 200

shocks from it. The consequence was, that the shaking of her head gradually decreased, till it intirely ceased ; that she was able at last to stand without any support ; and on leaving the room quite forgot one of her crutches, and walked to the kitchen with very little assistance from the other. That night she continued to be well and slept better than she had done for several months before, only about midnight she was seized with a faintifhness, and took notice of a strong sulphureous taste in her mouth ; but both faintness and that taste went off, upon drinking a little water. Next day, being electrified as before, her strength sensibly increased during the operation, and when that was over she walked easily with a stick, and could lift several pounds weight with her left hand, which had been so long paralytic before. The experiment was repeated on the third day ; by which time she had received in all upwards of 600 severe shocks. She then telling us that she had as much power in the side that had been affected as in the other, we believed it unnecessary to proceed farther as the electricity had already, to all appearance produced a complete cure. And indeed the patient continued to be well till the Sunday following, *viz.* about three days after the last operation ; but upon going that day to church, she probably caught cold ; for on Monday she complained of a numbness in her left hand and foot ; but, upon being again electrified, every symptom vanished, and she has been perfectly well ever since.

Coldingham, Nov. 1757.

Patrick Brydone.

That

That the above is a true and exact account of my case, and of the late wonderful cure wrought on me, is attested by

Elizabeth Foster.

I was eye witness to the electrical experiments made by my son on Elizabeth Foster, and saw with pleasure their happy effects. By the blessing of God accompanying them, from a weak, miserable, and at sometimes almost an insensible state, she was, in a very short time, restored to health and strength; of which the above is in every respect a true account.

Robert Brydone,
Minister of Coldingham.

*Extract of a Letter from Dr. Whytt to Dr. Pringle,
relating to this Account: Dated Edinburgh, 1 Dec.
1757.*

SOME days ago I had transmitted to me Mr. Brydone's account (inclosed) of the success of the electrical shocks in a paralytic patient, attested by the patient herself, and by Mr. Brydone's father, who is minister at Coldingham, in the shire of Berwick. At the same time I had a letter from the Reverend Mr. Allan, Minister of Eymouth (in the neighbourhood), informing me, that he had examined the patient particularly, and found Mr. Brydone's account to be perfectly true. He further informs me, that he never observed the electrical shock so strong from any machine, as from Mr. Brydone's. It seems, that gentleman has not only applied himself to the study of natural philosophy, but also of medicine.

Robert Whytt.

LI. *An Account of some fossile Fruits, and
other Bodies, found in the Island of Shepey.*
By James Parsons, M. D. F. R. S.

*To the Right Honourable the EARL of MACCLES-
FIELD, President of the Royal Society.*

My Lord,

Sept. 25, 1757.

Read Dec. 15. 1757. **B**EING ever desirous to promote the
business of this learned Society, I
could not lose the opportunity that presented, of lay-
ing before you an account, and drawings (*See TAB.*
XV. & XVI.), of a most curious parcel of fossil
fruits, and some other bodies, sent me from Shepey-
Island, by my ingenious friend Mr. Jacob, of Faver-
ham, Surgeon, and Fellow of the Antiquary So-
ciety.

I do not remember, that fossil seeds, or fruits, are
recorded in our Transactions, tho' many of other kinds
have places in them; nor indeed that the memoirs
of other academies have made mention of any such
fruits; and therefore, as these are chiefly pyritical,
and consequently liable to fall to pieces, I thought it
necessary to make drawings of them while in a sound
state, in order for engraving, if the Society shall think
fit; lest their being so subject to moulder away might
put it out of my power to preserve their forms. How-
ever, I have great hopes I shall be able to preserve the
greater part of them intire till they are shewed to the
Society.

In

In describing these bodies, we shall be obliged to make the best conjectures we can of some of them only; for several are sufficiently obvious to every naturalist, and easily known by comparing them to such recent fruits, as are frequent enough among us. Some of them are absolutely exotics; and indeed they are all rare and curious, and, in my humble opinion, well worth the notice of the Royal Society.

Doctor Woodward's catalogue *, which is so ample and full of all kinds of fossil bodies, has only a very few fruits; and these are only some hazle nuts found in different places, a few pine-cones, and laryxes; and one fruit, which was taken for an unripe nutmeg. In this collection before us they are all very different, and such as have not been seen before.

It will not be amiss, in this place, to give a short detail of such bodies as are capable of either being petrified themselves, or of leaving their impressions in stony matter. By being petrified, is meant being impregnated with stony, pyritical, or any other metal-lime or sparry matter; for there are innumerable specimens, wherein all these are apparent.

TESTACEOUS *and* CRUSTACEOUS ANIMALS.

The shelly matter of these is of so compact and dry a nature, that they will endure for ages: and if in a soil or bed where moisture has access, they will receive stony matter into their pores, and become ponderous in proportion to the quantity imbibed. If in a dry

* Since my writing this discourse, Dr. Mason informs me, that these are found no other than recent nuts and laryxes.

place, they will remain fair and sharp, suffering very little change by any length of time; whilst the flesh of these, being subject to putrification, is soon destroyed; and yet, according to circumstances that happen, some of these may be replaced in due form by stony particles. I have a gryphites, with the form of the fish in its place, as is the case in several of the oyster kinds. This may be occasioned by the shells being close, or nearly so, and stony matter gradually insinuating into their cavity, so as to fill up the whole.

W O O D.

The kinds of wood found fossil are very different: some are of a firmer texture than others: and this too is according to the places wherein they are deposited. Some I have seen so highly impregnated with a fine stony and pyritical matter, as to bear a polish like a pebble; some, tho' quite reduced to stone, yet preserving the fibrous appearance of the original state; and some which is found in boggy bottoms, being not at all changed, except in color: this is called bog oak, or bog deal, well known to country people in many places of these three kingdoms, who light themselves about their business with slips of this wood, cut on purpose instead of candles, as it burns with a clear and durable flame. It is remarkable, that altho' oak or fir shall lie ages immersed in water under ground, it shall not putrify; but acquire such sulphureous particles by lying in steep, in the bog-water, as to qualify it for this use. Other wood, deposited in marly ground, is found incruited over, trunk and branches, with a white crust; the wood remaining intire within.

At

At other times, wood thus incrustcd shall be eroded by the matter which covers it, having something acrimonious in its substance. We may add to these, clusters of the twigs of shrubs, and small wood, which we find flakes of, incrustcd with sparry or calcarious matter, in many places; parts of which are totally changed into that matter, whilst others are only enveloped with it.

BONES of ANIMALS.

We see, by every day's experience, that the human skeleton moulders to dust in a very few years, when buried in mould: so it does even in vaults, where the coffins are kept dry. In the first case, the moisture and salts of the earth divide and dissolve the texture of the bones; in the latter, those of the air, which gradually insinuate themselves into them, and at length destroy them. How long a skeleton whose bones are well dried and prepared, being totally deprived of its medullary substance, will last, as we now order them for anatomical purposes, we cannot say: but it may be reasonably conjectured, that they will undergo the fate of the softer kinds of wood, such as beech, which grows rotten in no great number of years; because their internal substance is spongy and cellular, and their crust is very thin, except about the middle of the bones of the arm and thigh, I mean the humerus and scæmur. The same destruction would happen, if bodies were deposited in a sandy soil; because water finds its way either by dripping downwards, or by springs underneath. But human skeletons have been found intire within a rock, where neither moisture

ture nor air could get at them. Mr. Minors, an eminent Surgeon and Anatomist of the Middlesex-hospital, when he was in the Army, at Gibraltar, saw an intire skeleton, standing upright, in a dry rock, part of which had been blown up with gunpowder, in carrying on some works in the fortifications, which left the skeleton quite exposed. Indeed, the bones of Elephants have been found in Shepey-Island, but much destroyed, several of which I have in my Collection; an account of which we have in the last volume but one * of our Transactions; their size and substance being so considerable, as to resist for a long time that decay which those of the human could not withstand. To these we may add the horns of large animals, as the elk, and others, which have been found in bogs, preserved as the bog-oak, &c. mentioned.

TEETH and PALATES of FISHES and other Animals.

These are of so hard and firm a texture, as to suffer no great change, wheresoever found; for we see, that no erosion appears in them, their enamel and its polish being intirely preserved; yet sometimes their roots will be found changed, especially in the yellow ones, having no enamel to guard them in their roots.

Parts of VEGETABLES.

The leaves of plants, whose fibres are firm and dry, will endure for a long time; but those of a succulent nature never can, as they putrify very soon. We see the leaves of ferns of several kinds, polypodium,

* Vol. xlviii.

tricomanes, and other capillary plants, with nodules of stone formed about them; flags, reeds, rushes, equisetum, and many such, of a firm texture, are found in slate and stone; and even the juli of trees are said to have been found fossil as well as their leaves.

SEEDS and FRUITS.

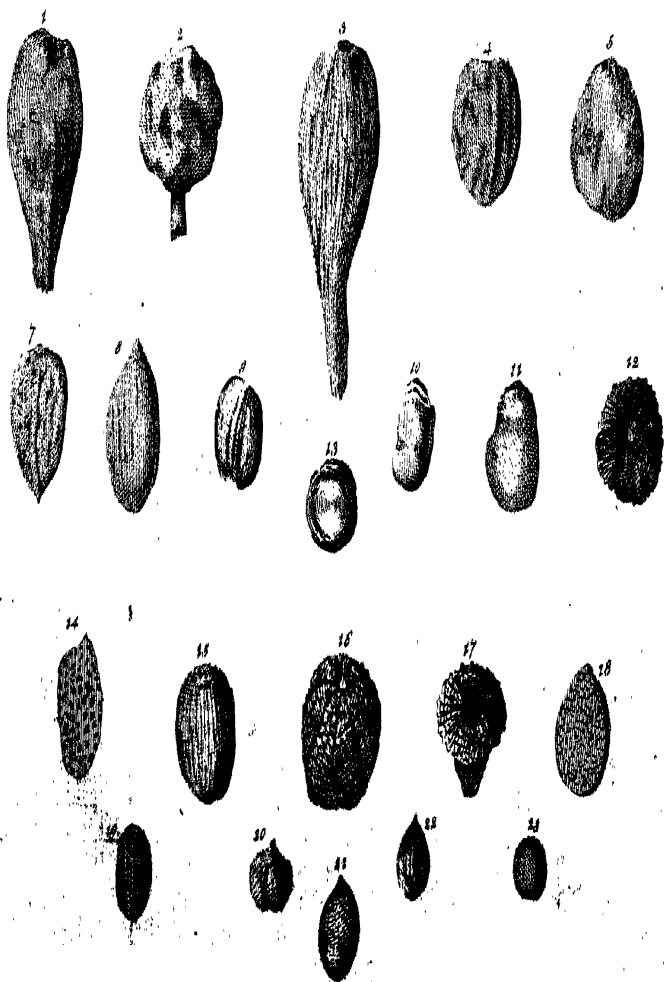
All seeds and the stones of fruits, having a firm texture, are also capable of being strongly impregnated with stony and pyritical matter; and I make no doubt but that the smaller seeds, if carefully looked for, might be found fossil, as well as these before you; such, I mean, as have a firmness in the covering; but being small, and mixt with the dirt, sand, and the like, probably is the reason of their being overlooked. Fruits of various kinds are found petrified; but this is only in their green state, when they are hard enough to endure till they are impregnated with stony or mineral particles. The rudiments of fruits, when once well formed, and a little advanced, are firm and acid: and the more remote they are from maturity, the more secure from putrefaction; and their acid juice is no small help to their preservation from growing soon rotten. But indeed, when the fruit advances in growth, the texture grows gradually more lax; the acid juices are now beginning to be replaced by saccharine or others more soft; the fibres are driven farther asunder, and they now arrive at their most ripe state: and the utmost maturity of fruits is the next step to putrefaction. Hence they are destroyed before stony or other particles can have time enough to impregnate them: and this is exactly the case with the flesh of animals of every-kind. The

husks and hard calyces of fruits, as well as their stones, are also susceptible of petrification.

If these fruits, which I have the honour to lay before you, are antediluvian, one would be apt to imagine they, in some measure, point out, with Dr. Woodward, the time of year in which the deluge began; which he thinks was in May: and yet this very opinion is liable to some objections; because altho' fruits capable of being petrified, from their green state, may be pretty well formed in May here, as well as in the same latitude elsewhere, in favour of this opinion; yet there are the stones of fruits, found fossil, so perfect, as to make one imagine they were very ripe, when deposited in the places where they are discovered; which would induce one to think the deluge happened nearer Autumn, unless we could think them the productions of more southern latitudes, where perhaps their fruits are brought to perfection before ours are well formed.

What follows is a catalogue of these fossil fruits &c. before you: and I should be glad, if any of the gentlemen would take the trouble of examining them, in order to assist in our conjectures about such of them, as appear doubtful: but first beg leave to insert the following remark:

I cannot omit an observation of Doctor Mason, Woodwardian professor, in this place; which is well worth notice, and indeed which I never attended to. It regards the impressions of fishes upon slate. 'Now there are several kinds of slate, which have such impressions upon them: in some there remains only the bare impression, without any part of the fish; in others the scales only, but retaining the intire form of the animal;



animal; and in others no part adheres to the slate, but the skeleton, or part of it, most commonly the spine. He says that he always observed, that the bones are never seen but upon the grey or blue slate, or their impressions; and that the scales or skin are to be found only upon the black stone or slate; which makes him conjecture, that something erosive in the grey slate destroys every part but the bony system; but that the black, being of a more soft and unctuous nature, preserves the scales, and often the very skin. This, however, must be referred to further observation.

T A B. XV.

Fig. 1. 3. These two bodies seem to be figs, petrified when hard and green; being, as I have just observed, then capable of receiving the pyritical particles, with which they are manifestly impregnated. One is more perfect in its form than the other; and they are now shooting their salts, and will soon fall to pieces.

Fig. 2. appears to be a Myrobalan, distinguished from the other species of that name by its round figure; and is called the belleric Myrobalan. It is nearly destroyed by the pyritical matter, and will not long remain whole.

Fig. 4. seems to be a species of Phaseolus, one of those especially distinguished by the fruits.
Fruētibus splendentibus nigris.

Fig. 5. Another Phaseolus.

Fig. 7. Another. See *Fig. 4.*

F f f 2

Fig. 8.

Fig. 8. Semen Cucurbitæ, a large species of American gourd.

Fig. 9. Coffee-berries.

Fig. 10, 11. Two species of Beans, very apparent.

Fig. 12. Unknown. This, however, appears to be a fruit, with the calyx running up, and embracing it, in its hard green state; being somewhat compressed on the upper part, as it lay confined in the earth.

Fig. 13. *An Staphilodendri species?* The learned and reverend Dr. Hales gave me, some years ago, a handful of the recent fruits, one or two of which are sent with this fossil one, for your consideration. He had them from Bengal, and called them, in the Indian name, Neermelis; and said the natives used them to fine down liquors.

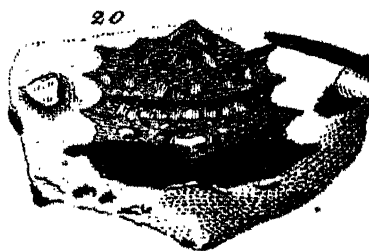
Fig. 14. A compressed pod of the Arachidna, or Underground-Pea. The full-grown pods are much larger, but of various sizes, as are other kinds. This, however, seems to have been, when deposited where it was found, not so far advanced. It has the reticulated surface, the apex on one side, and every other character of that fruit or seed-pod, but somewhat compressed.

Fig. 15. is evidently an Acorn. We have of this species here, and in America also.

Fig. 16. An exotic fruit, like a small melon; but uncertain. It is somewhat deformed by compression.

Fig. 17. This I took at first for a fruit; but now I rather believe it a Fungoides of a very pretty kind.

Fig. 18;



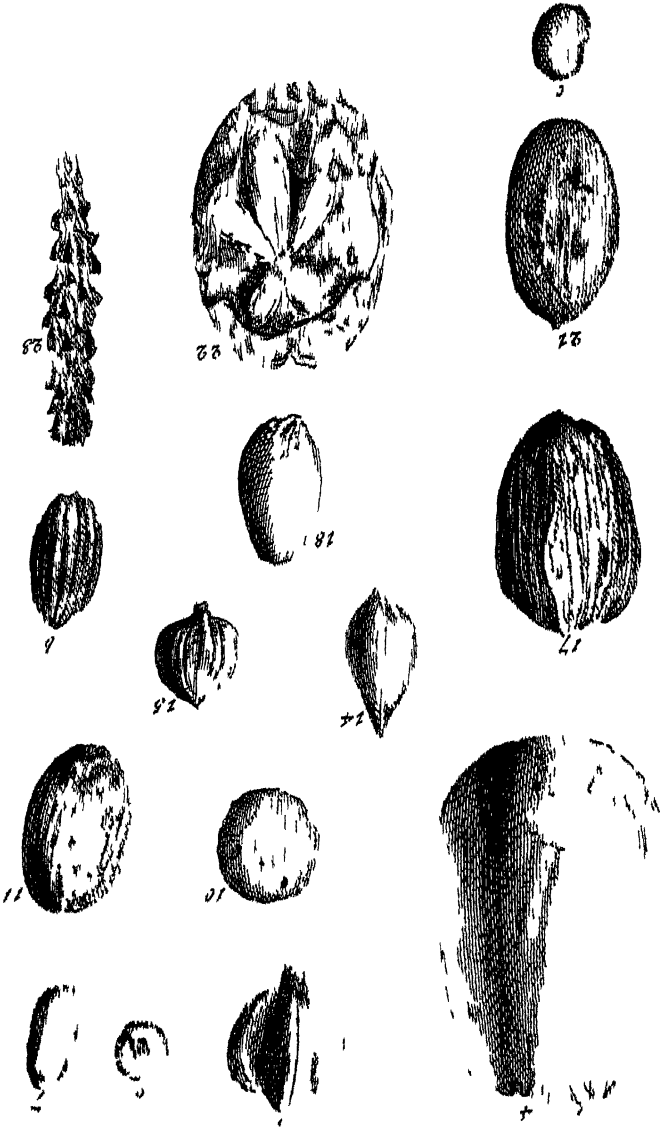


Fig. 18. *An Anguria?* I take it for a seed of a species of water-melon.

Fig. 19. seems a small plumb-stone.

Fig. 20. Unknown. The calyx seems to run up and embrace this fruit towards the apex.

Fig. 21. Unknown. This resembles an American seed, which I have in my collection, but do not know its name. Its apex is inclining to one side; and it appears to have had a strong pedicle.

Fig. 22. *An Lachryma Jobi?*

Fig. 23. A Cherry-stone.

T A B. XVI.

Fig. 1. *An Euonymi species?* If this be an Euonymus, it is not so far advanced as to form the seeds; and is therefore to be considered only in its progress from the flower towards seeding: which is the case in several of these, whose calyces appear still upon them, and hinder us from absolutely determining what they are.

Fig. 2. A berry of the Sapindus, or Soap-tree, of America, being not at all deformed, only having a little lump of pyrites upon it: but there is another quite free.

Fig. 3. *Hura Germen.* This is undoubtedly the young Sand-box, or fruit of the Hura, so well known for its beautiful form to the curious, who collect specimens of natural history; and seems to shew the time of the deluge.

Fig. 4. This, I think, is certainly the stone of an eastern Mango; such as comes over to us pickled,

pickled, and, the stone being opened on one side, is generally stuffed with spices.

Fig. 5. Euonymi latifolii species. This is a large species of Euonymus, perhaps of Clusius.

Fig. 6. This body seems to be a Milleped, or Wood-louse. It is turned round, the two extremities meeting; which is the attitude assumed by these animals, upon being in any-wise obstructed in their passage, or handled.

Fig. 7. A small long Bean, like our horse-bean; but longer than any we have in England.

Fig. 8. Unknown to me.

Fig. 9. A species of Horse-chestnut from America.

Fig. 10. The external husk of the fruit of the Sappindus, or Soap-tree.

Fig. 11. I cannot determine whether this be an Olive, or the yellow Myrobalan; but believe it the Myrobalan.

Fig. 12. An Palmæ species? It seems a small Palmarcoco.

Fig. 13, 14. unknown, as well as *fig. 15.*

Fig. 16. Unknown. The reason of the four last being not to be distinguished is, that they seem to be the buds of their several species, before they were perfectly formed. So that while some of the antediluvian productions are mature, others appear to be premature; and consequently one would be inclined to think them the inhabitants of places of different latitudes.

Fig. 17. A species of foreign Walnut, injured and compressed.

Fig. 18. A Plumb-stone.

Fig. 19. The claw of an American Crab; which, being

being on the opposite side of the mass containing the body, could not come in view with it at the same time.

Fig. 20. The body of the crab, with other parts, appearing thro' the stony matter that envelops it, which appears to be an induration of yellow clay.

Fig. 21. seems a long American Phaseolus. Part of the petrified husk is upon it.

Fig. 22. An American Echinite of the flat kind, much resembling that species which Rumphius calls *Echinus sulcatus primus*.

Fig. 23. *Arista cujusdam Graminis*. This body has all the characteristics of an ear of corn, or some species of grass, of which there are many.

This has been taken for a spine of an Echinus: but, as we are to consider its nearest resemblance to whatsoever body, we must conclude it as we have said. I never saw any spine in the least like it; but an ear of corn, ripe and dry, is as susceptible of being petrified, as a crustaceous animal, in every respect. Indeed the spiculæ of the ear, each arising from the grain, being very slender, are of course destroyed during the petrification; but the form of the ear is actually preserved, as much as the nature and circumstances of the thing will allow.

Fig. a. A manifest species of *Pediculus Marinus* crumpled up.

b. A Seed-vessel, given me by Mr. Da Costa, found in a clay-pit in Staffordshire.

c. *Cocculus Indicus*.

LII. Observations upon the Comet that appeared in the Months of September and October 1757, made at the Royal Observatory by Ja. Bradley, D. D. Astronomer Royal, F. R. S. and Member of the Royal Academy of Sciences at Paris.

Read Dec. 22, 1757. **I** Deferred to give an account of my observations upon the Comet that hath lately appeared, till I could settle the places of the stars with which it had been compared; several of them not being inserted in the British catalogue, and those which are, requiring some small corrections, which I have since made from my own observations.

When I first discovered this Comet, it appeared to the naked eye like a dull star of the 5th or 6th magnitude; but viewing it thro' a seven-foot Telescope, I could perceive a small Nucleus (surrounded, as usual, with a nebulous atmosphere), and a short tail extended in a direction opposite to the sun.

Some small stars then appearing in the field of the telescope with the Comet, I measured its distance from them with a Micrometer; and on September 12^d at 16^h 2' mean time, I found it to be 1° 13' 5" distant from a small star, whose right ascension was afterwards found to be 89° 49' 40" and declination 36° 11' 30" north: and near the same time the Comet was observed to be 43' 10" from another star, whose right ascension was 90° 20' 0" and declination 35° 12' 0" north.

Hence

Hence I collected, that the Comet's right ascension was $89^{\circ} 29' 10''$ and its declination $35^{\circ} 0' 20''$ north.

September $13^d 12^h 37'$ mean time (which is likewise made use of in the following observations), the Comet had the same right ascension with a small star, whose right ascension was $93^{\circ} 5' 30''$ and declination $34^{\circ} 36' 40''$ north; and it was about two minutes more northerly than the star. Hence the Comet's right ascension was $93^{\circ} 5' 30''$ and its declination $34^{\circ} 38' 40''$ north.

September $14^d 14^h 0'$ the Comet preceded θ Geminorum $1^{\circ} 31' 35''$ in right ascension, and was $11' 35''$ more southerly. The apparent right ascension of θ Geminorum was then $99^{\circ} 11' 40''$ and its declination $34^{\circ} 13' 25''$ north. Hence the right ascension of the Comet was $97^{\circ} 40' 5''$ and its declination $34^{\circ} 1' 50''$ north.

Sept. $17^d 13^h 0'$ a small star (whose right ascension was $109^{\circ} 55' 20''$ and declination $31^{\circ} 27' 40''$) preceded the Comet $47' 10''$ in right ascension, and was $12' 30''$ more northerly. Hence the Comet's right ascension was $110^{\circ} 42' 40''$ and its declination $31^{\circ} 15' 10''$ north.

Sept. $19^d 15^h 17'$ a star (whose right ascension was $118^{\circ} 29' 40''$ and declination $28^{\circ} 9' 45''$) preceded the Comet $1^{\circ} 14' 0''$ in right ascension, and was more southerly $15' 45''$. Hence the Comet's right ascension was $119^{\circ} 43' 40''$ and declination $28^{\circ} 25' 30''$ north.

Sept. $23^d 15^h 57'$ a star (whose right ascension was $134^{\circ} 55' 45''$ and declination $22^{\circ} 15' 55''$ north) preceded the Comet $12' 30''$ in right ascension, and

was $29^{\circ} 0''$ more northerly. Hence the Comet's right ascension was $135^{\circ} 8' 15''$ and its declination $21^{\circ} 46' 55''$ north.

Sept. $24^d 15^h 21'$ the Comet had the same declination with a small star that preceded it $10' 15''$ in right ascension. This star's right ascension was afterwards found to be $138^{\circ} 13' 45''$ and its declination $20^{\circ} 5' 20''$. Hence the Comet's right ascension was $138^{\circ} 24' 0''$ and its declination $20^{\circ} 5' 20''$ north.

Sept. $28^d 16^h 22'$ the Comet followed Regulus $1^{\circ} 7' 12''$ in right ascension, and was $14' 45''$ more northerly. The right ascension of Regulus being then $148^{\circ} 51' 13''$ and its declination $13^{\circ} 8' 35''$ north; the Comet's right ascension was $149^{\circ} 58' 25''$ and its declination $13^{\circ} 23' 20''$ north.

Sept. $30^d 16^h 24'$ ϵ Leonis (whose right ascension was $155^{\circ} 0' 10''$ and declination $10^{\circ} 32' 53''$ north) followed the Comet $18' 45''$ in right ascension, and was $7' 53''$ more northerly. Hence the Comet's right ascension was $154^{\circ} 41' 25''$ and its declination $10^{\circ} 25' 0''$ north.

October $2^d 16^h 48'$ the 37th star Sextantis. Hevel. in the British Catalogue (whose right ascension was $158^{\circ} 21' 25''$ and declination $7^{\circ} 38' 40''$ north) preceded the Comet $32' 50''$ in right ascension, and was $3' 20''$ more southerly. Hence the Comet's right ascension was $158^{\circ} 54' 15''$ and its declination $7^{\circ} 42' 0''$ north.

October $3^d 16^h 45'$ α Leonis (whose right ascension was $162^{\circ} 2' 15''$ and declination $7^{\circ} 24' 0''$ north) followed the Comet $1^{\circ} 12' 55''$ in right ascension, and was $56' 40''$ more northerly. Hence the Comet's right ascension was $160^{\circ} 49' 20''$ and its declination $6^{\circ} 27' 20''$ north.

October 4^d 17^h 0' *d* Leonis (whose right ascension was $162^{\circ} 0' 15''$ and declination $4^{\circ} 54' 57''$ north) preceded the Comet $40' 15''$ in right ascension, and was more southerly $20' 53''$. Hence the Comet's right ascension was $162^{\circ} 40' 30''$ and its declination $5^{\circ} 15' 50''$ north.

October 7^d 16^h 54' the 79th Leonis in the British Catalogue (whose right ascension was $167^{\circ} 53' 37''$ and declination $2^{\circ} 44' 15''$ north) followed the Comet $13' 0''$ in right ascension, and was more northerly $38' 35''$. Hence the Comet's right ascension was $167^{\circ} 40' 37''$ and its declination $2^{\circ} 5' 40''$ north.

October 8^d 16^h 53' the Comet preceded ν Leonis $1^{\circ} 53' 30''$ in right ascension, and was $37' 20''$ more northerly. The right ascension of this star was $171^{\circ} 7' 45''$ and its declination $0^{\circ} 30' 55''$ north; therefore the Comet's right ascension was $169^{\circ} 14' 15''$ and its declination $1^{\circ} 8' 15''$ north.

October 11^d 16^h 52' the Comet followed ν Leonis $2^{\circ} 33' 30''$ in right ascension, and appeared $1^{\circ} 55' 5''$ more southerly; but it being near the horizon, the difference of right ascension must have been contracted by refraction about $1' 5''$, and the difference of declination about $1' 30''$: so that the corrected right ascension of the Comet was $173^{\circ} 42' 20''$ and its declination $1^{\circ} 25' 40''$ south.

Immediately after this observation a fog arose, which prevented me from repeating it; and several mornings following proving hazy or cloudy, I could not see the Comet again till October 18th, about an hour and a quarter before sun-rising; when the twilight being strong, and the Comet low, it appeared very faint. However, I was unwilling to omit the

opportunity of determining its place, as near as I could, by a single observation, in the following manner.

At $6^h 59' 54'' \frac{1}{2}$ sidereal time, I observed the passage of the Comet over the perpendicular wire of my equatorial Sector; then leaving the instrument in the same position till the next evening, I observed, that at $22^h 8' 15''$ sidereal time, the 17th star of Eridanus in the British Catalogue passed over the same wire (or horary circle) $9' 30''$ more southerly than the Comet. And at $23^h 45' 36''$ sidereal time the star marked *b* in Eridanus passed, $19' 55''$ more northerly than the Comet.

I found that the situation of my instrument was not sensibly altered between the 18th and 19th of October; for the transits and the difference of declination of the same stars being observed with it again on the 19th of October, they agreed very well with those that were taken the preceding night. It may therefore be supposed, that the position of the instrument continued the same likewise during the time of the foregoing observations.

The right ascension of the 17th star of Eridanus being $49^\circ 39' 10''$ and its declination $5^\circ 55' 25''$ south; and the right ascension of *b* of Eridanus being $73^\circ 59' 25''$ and its declination $5^\circ 25' 10''$ south; I collected, that when the Comet passed the wire (or horary circle) which was October 17^d 17^h 12' mean time, its right ascension was $182^\circ 34' 0''$ and its declination $5^\circ 45' 35''$ south.

The last time that I saw the Comet was on the 19th of October in the morning; but it then appeared so faint, that I could not observe its place. Its elongation from the sun was then but about 20 degrees;

degrees; and from that day to the present it hath always been less; which is the principal reason why it was invisible to us at the time when it was in its perihelion, and hath remained so ever since. The elongation will indeed soon become greater, and yet it is probable that we shall not be able to see the Comet again; because its real distance from the sun will be greater than it was when I first saw it, and it will be also four times further from us than it was at that time.

The Comet kept nearly at the same distance from the earth for ten or twelve days together after I first saw it; but its brightness gradually increased then, because it was going nearer to the sun. Afterwards, when its distance from the earth increased, altho' it continued to approach the sun, yet its lustre never much exceeded that of stars of the second magnitude, and the tail was scarce to be discerned by the naked eye.

All the forementioned observations were made with a Micrometer in a seven-foot Tube, excepting those of the 3d, 11th, and 17th days of October, which were taken with a curious Sector constructed for such purposes by the late ingenious Mr. George Graham; of which Dr. Smith has given a very exact description in his third book of Optics.

Supposing the Trajectory of this Comet to be parabolic, I collected from the foregoing observations, that its motion round the sun is *direct*, and that it was in its *perihelion* October the 21st, at 7^h 55' mean (or equated) time at Greenwich. That the inclination of the plane of its Trajectory to the ecliptic is 12° 50' 20"; the place of the descending Node 8 4' 12'

12' 50"; the place of the Perihelion at $2^{\circ} 58' 0''$; the distance of the Perihelion from the descending Node $88^{\circ} 45' 10''$; the Logarithm of the Perihelion distance 9.528328; the Logarithm of the diurnal motion 0.667636.

From these Elements (which are adapted to Dr. Halley's general Table for the Motion of Comets in parabolic Orbits), I computed the places of this Comet for the respective times of the foregoing observations, as in the following table; which contains likewise the longitudes and latitudes deduced from the observed right ascensions and declinations, and also the differences between the computed and observed places. These differences (no-where exceeding $40''$) shew, that the elements here set down will be sufficient to enable future astronomers to distinguish this Comet upon another return; but as they do not correspond with the elements of the orbit of any other Comet hitherto taken notice of, we cannot determine at present the period thereof.

Greenwich, 1757- Mean Time.		Comet. Long. Observ.			Latit. Observ.			Long. Comp.			Latit. Comput.			Diff. Long.	Diff. Latit.
d.	h.	S.	o.	'	''	o.	'	''	o.	'	''	o.	'	''	''
Sept.		II	29	34	13	11	32	16	No.			11	32	20	No.
12	16	2													4
13	12	37				11	12	13				11	12	11	2
14	14	0				10	44	3				10	43	43	20
17	13	0	17	49	40	9	3	38				9	3	11	20
19	15	17	26	6	8	7	36	49				7	36	30	19
23	15	57	8	11	19	4	33	38				4	33	32	6
24	15	21	14	44	19	3	49	37				3	49	39	2
28	16	22	27	23	43	1	3	44	No.			1	3	52	8
30	16	24	22	45	43	0	5	30	So.			0	5	17	13
Octob.		2	16	48	7	37	43					1	5	32	18
3	16	45	9	51	36	1	32	22				1	31	55	27
4	17	0	12	1	4	1	56	42				1	56	23	19
7	16	54	17	51	3	2	56	48				2	56	24	24
8	16	53	19	39	45	3	13	7				3	12	28	39
11	16	52	24	47	22	3	48	49				3	49	29	40
17	17	12	24	4	38	4	15	42	So.			4	15	2	40

LIII. *The Resolution of a General Proposition for Determining the horary Alteration of the Position of the Terrestrial Equator, from the Attraction of the Sun and Moon: With Some Remarks on the Solutions given by other Authors to that difficult and important Problem. By Mr. Tho. Simpson, F. R. S.*

Read Dec. 22, 1757. **S**INCE the time, that that excellent Astronomer, my much honoured friend Dr. Bradley, published his observations and discoveries concerning the inequalities of the precession of the equinox, and of the obliquity of the ecliptic, depending on the position of the lunar nodes, mathematicians, in different parts of Europe, have set themselves diligently to compute, from physical principles, the effects produced by the sun and moon, in the position of the terrestrial equator; and to examine whether these effects do really correspond with the observations

Two papers on this subject have already appeared in the Philosophical Transactions; in which the authors have shewn evident marks of skill and penetration. There is, nevertheless, one part of the subject, that seems to have been passed over without a due degree of attention, as well by both those gentlemen, as by Sir Isaac Newton himself.

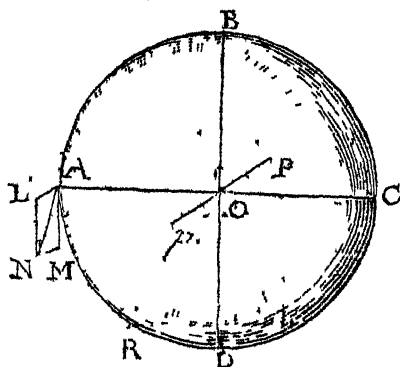
This part, which, upon account of physical difficulties, is indeed somewhat slippery and perplexing, I shall make the principal subject of this essay.

GENE-

GENERAL PROPOSITION.

Supposing an homogeneous sphere $OABCD$ (Fig. I.) revolving uniformly about its centre, to be acted on at the extremity A of the radius OA , in a direction AL perpendicular to the plane of the equator $ABCD$, and parallel to the axis of rotation Pp , by a given force, tending to generate a new motion of rotation at right angles to the former; It is proposed to determine the change, that will arise in the direction of the rotation in consequence of the said force.

FIG. I.



Let F denote the given force, whereby the motion about the axis Pp is disturbed, supposing f to represent the centrifugal force of a small particle of matter in the circumference of the equator, arising from the sphere's rotation; and let the whole number of such particles, or the content of the sphere, be denoted by c : let also the momentum of rotation of the whole sphere, or of all the particles, be supposed, in proportion to the momentum of an equal number

of particles, revolving at the distance OA of the remotest point A , as n is to *unity*.

It is well known, that the centripetal force, whereby any body is made to revolve in the circumference of a circle, is such, as is sufficient to generate all the motion in the body, in a time equal to *that*, wherein the body describes an arch of the circumference, equal in length to the radius. Therefore, if we here take the arch $AR = OA$, and assume m to express the time, in which that arch would be uniformly described by the point A , the *motion* of a particle of matter at A (whose central force is represented by f) will be equal to *that*, which might be uniformly generated by the force f , in the time m ; and the motion of as many particles (revolving, all, at the same distance) as are expressed by cn (which, by hypothesis, is equal to the momentum of the whole body), will, consequently, be equal to the momentum, that might be generated by the force $f \times cn$, in the same time m . Whence it appears, that the momentum of the whole body about its axis Pp is in proportion to the momentum generated in a given particle of time m' , by the given force F in the direction AL , as $ncf \times m$ is to $F \times m'$, or, as *unity* to $\frac{F}{ncf} \times \frac{m'}{m}$. (because the quantities of motion produced by unequal forces, in unequal times, are in the ratio of the forces and of the times, conjunctly). Let, therefore, AL be taken in proportion to AM , as $\frac{F}{ncf} \times \frac{m'}{m}$ is to *unity* (supposing AM to be a tangent to the circle $ABCD$ in A), and let the parallelogram $AMNL$ be completed; drawing also the diagonal AN ; then, by the

the composition of forces, the angle NAM (whose tangent, to the radius OA , is expressed by $OA \times \frac{F}{n c f} \times \frac{m'}{m}$) will be the change of the direction of the rotation, at the end of the aforesaid time (m'). But, this angle being exceeding small, the tangent may be taken to represent the measure of the angle itself; and, if Z be assumed to represent the arch described by A , in the same time (m') about the center O , we shall also have $\frac{m'}{m} = \frac{Z}{AR} = \frac{Z}{AO}$, and consequently $OA \times \frac{F}{n c f} \times \frac{m'}{m} = Z \times \frac{F}{n c f}$. From whence it appears, that the angle expressing the change of the direction of the rotation, during any small particle of time, will be in proportion to the angle described about the axe of rotation in the same time, as $\frac{F}{n c f}$ is to unity. *Q. E. I.*

Altho', in the preceding proposition, the body is supposed to be a perfect sphere, the solution, nevertheless, holds equally true in every other species of figures, as is manifest from the investigation. It is true, indeed, that the value of n will not be the same in these cases, even supposing those of c , f and F to remain unchanged; except in the spheroid only, where, as well as in the sphere, n will be $= \frac{2}{3}$; the momentum of any spheroid about its axis being 2-5ths of the momentum of an equal quantity of matter placed in the circumference of the equator, as is very easy to demonstrate.

But to shew now the use and application of the general proportion here derived, in determining the regress of the equinoctial points of the terrestrial

But it is demonstrated by Sir Isaac Newton, and by other authors, that the force of all the particles, or of all the matter in the whole spheroid $APap$, to turn it about its center, is equal to $\frac{1}{15}$ th of the force of a quantity of matter, placed at A , equal to the excess of the matter in the whole spheroid above *that* in the inscribed sphere, whose axis is Pp . Now this excess (assuming the ratio of π to 1, to express *that* of the area of a circle to the square of the radius) will be truly represented by $\frac{4\pi}{3} \times OP \times \overline{OA^2 - OP^2}$; and, consequently, the force of all the matter in the whole earth, by $\frac{3tt}{TT} \times \frac{AK}{OA} \times \frac{OK}{OA} \times \frac{4\pi}{15} \times OP \times \overline{OA^2 - OP^2}$. Let, therefore, this quantity be now substituted for F , in the general formula $\frac{F}{ncf}$, writing, at the same time, $\frac{4\pi}{3} \times OA^2 \times OP$, and $\frac{2}{T}$, in the place of their equals c and n ; by which means we have (here)

$$\frac{F}{ncf} = \frac{3tt}{2TT} \times \frac{OA^2 - OP^2}{OA^2} \times \frac{AK \times OK}{OA^2}.$$

Put the given quantity $\frac{3tt}{2TT} \times \frac{OA^2 - OP^2}{OA^2} = k$; and let the angle $E Ae$ represent the horary alteration of the position of the terrestrial equator, arising from the force F (here determined), and let the arch Ee be the regress of the equinoctial point E , corresponding thereto: then, in the triangle $E Ae$ (considered as spherical) it will be $\sin. e : \sin. AE (:: \sin. EAe : \sin. Ee)$
 $:: EAe : Ee \left(= \frac{\sin. AE \times EAe}{\sin. E} \right) = k \times \frac{\sin. AE}{\sin. E} \times$
 $\frac{AK \times OK}{OA^2} = k' \times \frac{\sin. AE \times \cos. AH \times \sin. AH}{\sin. E}.$ But in the triangle EHA , right-angled at A (where HA is supposed

supposed to represent the sun's declination, AE his right ascension, and HE his distance from the equinoctial point E *) we have (*per spherics*)

$$\begin{aligned} \text{fin. AE} : 1 (\text{rad.}) &:: \text{co-t. E} : \text{co-t. AH}, \\ \overline{\text{fin. AH}^2} : \overline{\text{fin. EH}^2} &:: \overline{\text{fin. E}^2} : 1^2 (\text{rad.}^2) \end{aligned}$$

From whence we get, $\text{fin. AE} \times \text{co-t. AH} \times \overline{\text{fin. AH}^2} = \overline{\text{fin. EH}^2} \times \text{co-t. E} \times \overline{\text{fin. E}^2}$. But $\text{co-t. AH} \times \text{fin. AH} = \text{co-f. AH} \times 1 (\text{rad.})$, and $\text{co-t. E} \times \text{fin. E} = \text{co-f. E} \times 1 (\text{rad.})$: therefore $\text{fin. AE} \times \text{co-f. AH} \times \text{fin. AH} = \overline{\text{fin. EH}^2} \times \text{co-f. E} \times \text{fin. E}$; and, consequently, $k \times \frac{\text{fin. AE} \times \text{co-f. AH} \times \text{fin. AH}}{\text{fin. E}} = k \times \text{co-f. E} \times \overline{\text{fin. EH}^2} (= Ee)$.

Let, now, the sun's longitude EH be denoted by Z (considered as a flowing quantity); then, $\overline{\text{fin. Z}^2}$ being $= \frac{1}{2} - \frac{1}{2} \text{co-f. } 2Z$, we shall have $k \times \text{co-f. E} \times \overline{\text{fin. EH}^2} = \frac{1}{2} k \times \text{co-f. E} \times 1 - \text{co-f. } 2Z$. But the angle described about the axe of rotation Pp, in the time that the sun's longitude is augmented by the particle \dot{Z} , will be $= \frac{T}{t} \times \dot{Z}$. Therefore (by the general proposition) we have, as $1 : \frac{1}{2} k \times \text{co-f. E} \times 1 - \text{co-f. } 2Z :: \frac{T}{t} \times \dot{Z} : \frac{1}{2} k \times \frac{T}{t} \times \text{co-f. E} \times \dot{Z} - \dot{Z} \text{co-f. } 2Z$, the true regrefs of the equinoctial point E, during

* No error arises from considering the triangles EAs and AEH, as being formed on the surface of a sphere, tho' the earth itself is not accurately such. The angle (EAs) representing the effect of the solar force, is properly referred to the surface of a sphere; therefore (after the measure thereof is truly determined) the figure APap is itself taken as a sphere, in order to avoid the trouble of introducing a new scheme.

that

that time: whose fluent, $\frac{1}{2} k \times \frac{T}{t} \times \text{co-f. } E \times \overline{Z} = \frac{1}{2} \text{fin. } 2 \overline{Z}$, will consequently be the total regrefs of the point E, in the time that the sun, by his apparent motion, describes the arch HE or Z; which, on the sun's arrival at the solstice, becomes barely $= \frac{1}{2} k \times \frac{T}{t} \times \text{co-f. } E \times \text{an arch of } 90^\circ$: the quadruple whereof, or $\frac{1}{2} k \times \frac{T}{t} \times \text{co-f. } E \times 360^\circ \left(= \frac{3t}{4T} \times \frac{OA^2 - OP^2}{OA^2} \times \text{co-f. } E \times 360^\circ \right)$ is therefore the whole annual precession of the equinox caused by the sun. This, in numbers (taking $\frac{OP}{OA} = \frac{229}{230}$) comes out $\frac{3}{4 \times 366\frac{1}{2}} \times 2' \times 0.917176 \times 360^\circ = 21'' 6'''$.

The very ingenious M. Silvabelle, in his essay on this subject, inserted in the 48th volume of the Philosophical Transactions, makes the quantity of the annual precession of the equinox, caused by the sun, to be the half, only; of what is here determined. But this gentleman appears to have fallen into a twofold mistake. First, in finding the *momenta of rotation* of the terrestrial spheroid, and of a very slender ring, at the equator thereof; which *momenta* he refers to an axis perpendicular to the plane of the sun's declination, instead of the proper axe of rotation, standing at right angles to the plane of the equator. The difference, indeed; arising from thence, with respect to the spheroid (by reason of its near approach to a sphere) will be inconsiderable; but, in the ring, the case will be quite otherwise; the equinoctial points thereof being made to recede just twice as fast

as they ought to do. This may seem the more strange, if regard be had to the conclusions, relating to the nodes of a satellite, derived from this very assumption. But, that these conclusions are true, is owing to a second, or subsequent mistake, at Art. 27; where the measure of the sun's force is taken the half, only, of the true value; by means whereof the motion of the equinoctial points of the ring is reduced to its proper quantity, and the motion of the equinoctial points of the terrestrial spheroid, to the half of what it ought to be.

That expert geometrician M. Cha. Walmsley, in his Essay on the Precession of the Equinox, printed in the last volume of the Philosophical Transactions, has judiciously avoided all mistakes of this last kind, respecting the sun's force, by pursuing the method, pointed out by Sir Isaac Newton; but, in determining the effect of that force, has fallen into others, not less considerable than those above adverted to.

In his third Lemma, the momentum of the whole Earth, about its diameter, is computed on a supposition, that the momentum or force of each particle is proportional to its distance from the axis of motion, or barely as the quantity of motion in such particle, considered abstractedly. No regard is, therefore, had to the lengths of the unequal levers, whereby the particles are supposed to receive and communicate their motion: which, without doubt, ought to have been included in the consideration.

In his first proposition, he determines, in a very ingenious and concise manner, the true annual motion of the nodes of a ring (or of a single satellite) at the earth's equator, revolving with the earth itself, about
its

its center, in the time of one siderial day. This motion he finds to be $= \frac{3 \text{ co-f. } 23^{\circ} 29'}{4 \text{ rad.}} \times \frac{1}{366\frac{1}{4}} \times 360^{\circ}$. Then, in order to infer from thence, the motion of the equinoctial points of the earth itself, he, first, diminishes that quantity, in the ratio of 2 to 5: Because (as is demonstrated by Sir Isaac Newton in his 2d Lemma) the whole force of all the particles situated without the surface of a sphere, inscribed in the spheroid, to turn the body about its center, will be only 2-5ths of the force of an equal number of particles uniformly disposed round the whole circumference of the equator, in the fashion of a ring. The quantity $\left(\frac{3 \text{ co-f. } 23^{\circ} 29'}{4 \text{ rad.}} \times \frac{2}{5} \times \frac{1}{366\frac{1}{4}} \times 360^{\circ} \right)$ thus arising, will, therefore, express the true motion of the equinoctial points of a ring, equal in quantity of matter to the excess of the whole earth above the inscribed sphere, when the force whereby the ring tends to turn about its diameter is supposed equal to the force whereby the earth itself tends to turn about the same diameter, in consequence of the sun's attraction. Thus far our author agrees with Sir Isaac Newton; but, in deriving from hence the motion of the equinoctial points of the earth itself, he differs from him; and, in the corollary to his third Lemma, assigns the reasons, why he thinks Sir Isaac Newton, in this particular, has *wandered a little from the truth*. Instead of diminishing the quantity above exhibited (as Sir Isaac has done) in the ratio of all the motion in the ring to the motion in the whole earth, he diminishes it in the ratio of the motion of all the matter above the surface of the inscribed sphere to the motion of the whole earth: which matter, tho' equal

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to that of the ring, has nevertheless a different momentum, arising from the different situation of the particles in respect to the axis of motion.

But since the aforesaid quantity, from whence the motion of the earth's equinox is derived, as well by this gentleman, as by Sir Isaac Newton, expresses truly the annual regrefs of the equinoctial points of the ring (and not of the hollow figure formed by the said matter, which is greater, in the ratio of 5 to 4) it seems, at least, as reasonable to suppose, that the said quantity, to obtain from thence the true regrefs of the equinoctial points of the earth, ought to be diminished in the former of the two ratios above specified, as that it should be diminished in the latter. But, indeed, both these ways are defective, even supposing the momenta to have been truly computed; the ratio, that ought to be used here, being that of the momenta of the ring and earth about the proper axe of rotation of the two figures, standing at right-angles to the plane of the ring and of the equator. Now this ratio, by a very easy computation, is found to be as $\overline{230}^2 - \overline{229}^2$ to $\frac{2}{7}$ of $\overline{230}^2$; whence the quantity sought comes out $= \frac{3 \text{ co-f. } 23^\circ 29'}{4 \text{ rad.}}$

$\times \frac{1}{366\frac{1}{4}} \times \frac{\overline{230}^2 - \overline{229}^2}{230^2} \times 360^\circ = 21'' 6'''$: which is the same that we before found it to be, and the double of what this author makes it.

What has been said hitherto, relates to that part of the motion only, arising from the force of the sun. It will be but justice to observe here, that the effect of the moon, and the inequalities depending on the position of her nodes, are truly assigned by both the gentlemen

gentlemen above-named; the ratio of the diameters of the earth, and the density of the moon being so assumed, as to give the maxima of those inequalities, such as the observations require: in consequence whereof, and from the law of the increase and decrease (which is rightly determined by theory, tho' the absolute quantity is not) a true solution, in every other circumstance, is obtained.

The freedom, with which I have expressed myself, and the liberty I have here taken, to animadvert on the works of men, who, in many places, have given incontestible proofs of skill and genius, may, I fear, stand in need of some apology. 'Tis possible I may be thought too peremptory. Indeed, I might have delivered my sentiments with more caution and address: but, had not I imagined myself quite clear in what has been advanced, from a multitude of concurrent reasons, I should have thought it too great a presumption to have said any thing at all here, on this subject. The great regard I have for this Society, of which I have the honour to be a member, will, I hope, be considered as the motive for my having attempted to rectify some oversights, that have occurred in the works of this learned body.

LIV. *Remarks upon the Heat of the Air in July 1757. in an Extract of a Letter from John Huxham, M.D. F.R.S. to William Watson, M.D. F.R.S. dated at Plymouth 19th of that Month. With additional Remarks by Dr. Watson.*

Read Dec. 22, 1757. “ **F**ROM the beginning of June last we have had a very dry season, generally very warm, and sometimes excessively hot. From the 7th to the 14th of this month the heat was violent; greater, indeed, than has been known here in the memory of man. I have talked with several persons, who have lived a considerable time in Jamaica, Gibraltar, and Minorca; and they severally assert, that they never felt such intense heat in any of those places. Upon the 11th, 12th, and 13th of this month, Fahrenheit’s thermometer, in the shade, about three o’clock in the afternoon, was at 87; nay, upon the 12th it was even above 88. Abundance of people have suffered very severely from these excessive heats: putrid, bilious, pe- techial, nervous fevers, are exceedingly common every-where. Dysenteries, hæmorrhages, most profuse sweats, affect not only those in fevers, but a vast many others. The days and nights were so intolerably hot, that little or no sleep was to be gotten day or night. The wind we had, like the Campsin, actually blew hot, tho’ strong.

Upon

“ Upon the 15th, about seven at night, at Fal-
 “ mouth, Penryn, Tiuro, and thereabouts, a pretty
 “ smart shock of an earthquake was felt, attended
 “ with a hollow rumbling noise, throwing down
 “ pewter, china-ware, and such-like. The tinnors
 “ felt it eighty fathom under ground. No great
 “ damage however was done. The day before we
 “ had, about eleven o’clock before noon, a most
 “ violent hurricane, which lasted five or six minutes,
 “ attended with a heavy shower.”

Thus far Dr. Huxham.

The heat of the air at London, during the period above-mentioned, was much greater than has been usually observed in these high latitudes; tho’ it was never quite so severe here as at Plymouth. The following table exhibits the degrees of the heat, taken here upon the respective days, about four o’clock in the afternoon, by a Fahrenheit’s thermometer. The instrument was placed in the shade; and the accuracy of the observer, who favoured me with his minutes, is not to be questioned.

1757. July	5	—	—	75
	6	—	—	78
	7	—	—	75½
	8	—	—	78
	10	—	—	80¼
	11	—	—	83¾
	12	—	—	80¾
	13	—	—	80
	14	—	—	85
	15	—	—	81
	16	—	—	73

From

From hence it appears, that the air at London was, upon several days, hotter than it had been observed at Madeira for ten years together : for, by Dr. Thomas Heberden's observations, mentioned in the Philosophical Transactions, the heat of the air at Madeira, during that period, was never but once at 80.

William Watson.

LV. *Remarks upon the Letter of Mr. John Ellis, F.R.S. to Philip Carteret Webb, Esq; F.R.S. printed in the Philosophical Transactions, Vol. xlix. Part ii. p. 806. By Mr. Philip Miller, F.R.S.*

Read Dec. 15. 1757. **T**HE paper of mine, which was read before the Royal Society on the 8th of May 1755, and afterward printed in the xlixth volume of the Philosophical Transactions *, was written at the request of Mr. Watson ; who informed me, that a letter from the Abbé Mazeas to the reverend Dr. Hales had been communicated to the Royal Society, in which it was mentioned, that the Abbé Sauvages had made a discovery of the juice of the Carolina Toxicodendron staining linen of a permanent black. But Mr. Watson said, that the letter, he thought, required a careful perusal before it was printed ; and he wished I would confirm it. I told

* Part I. p. 161.

which I had raised a few years before from seeds, which were sent by Mr. Catesby from Carolina.

And altho' this shrub had not been reduced to any genus before, yet it had been some years growing in the gardens of the Bishop of London at Fulham, at Mr. Reynardson's at Hillenden, Mr. Darby's at Hoxton, and in the Chelsea garden, which were raised from seeds sent by Mr. Banister from Virginia; two of which were growing at Chelsea in the year 1722, when the care of that Garden was intrusted to me.

The first intimation I had of the American shrub being the same with Dr. Kœmpfer's true varnish-tree, was from the late Dr. William Sherard, in the year 1726, when that gentleman desired me to bring him a specimen of the American *Toxicodendron* from the Chelsea garden; which I accordingly did: and then the Doctor, and Dr. Dillenius, compared it with a dried specimen in the collection of the former, which was gathered in Japan, and which, if I remember right, he told me he received from Dr. Kœmpfer some years before. It appeared to those two gentlemen, that they were the same; and their skill in the science of botany was never doubted.

About a year after this, I carried a specimen of the American *Toxicodendron* to an annual meeting of some botanists at Sir Hans Sloane's in Bloomsbury; where there were present Mr. Dale of Braintree, Mr. Joseph Miller, Mr. Rand, and some others; which was then compared with Dr. Kœmpfer's specimen, whose collection Sir Hans Sloane had purchased: and it was the opinion of every one present, that they were the same. Nor has any one doubted of their being so, who has compared the American shrub
with

with Kœmpfer's figure and description of his true varnish-tree, but Mr. Ellis.

And now give me leave to examine his reasons for differing in opinion from every late botanist, who has mentioned this shrub.

He says, that the midrib, which supports the lobe leaves, is quite smooth in the poison-ash, as is also the under side of the leaves; whereas Dr. Kœmpfer, in his description of the midrib of the true varnish-tree, calls it *læviter lanuginoso*; and in his description of the lobes or *pinnæ* he says, they are *basi inequaliter rotunda*; whereas those of the poison-ash come to a point at their footstalks nearly equal to that at the top. These characters, Mr. Ellis thinks, are sufficient to prove, that they are different plants: and he blames Dr. Dillenius for having omitted these necessary characters in his description of it; and supposes this must have misled the accurate Linnæus, who quotes his synonyma.

But as Dr. Linnæus is possessed of Kœmpfer's book, he would little have deserved the appellation of accurate in this particular, had he not consulted the original, but trusted to a copy. But this I know he has done, and is as well assured, that the plants in question are the same, as Mr. Ellis can be of the contrary.

But here I must observe, that the branch, from which Dr. Kœmpfer's figure is taken, is produced from the lower part of a stem, which seems to have been cut down, and not from a flowering branch; and it is not improbable, that his description may have been taken from the same branch: and if this be the case, it is easy to account for the minute dif-

ferences mentioned by Mr. Ellis ; for it would not be difficult to produce instances of hundreds of different trees and shrubs, whose lower and upper branches differ much more in the particulars mentioned by Mr. Ellis, than the figure and description given by Kœmpfer do from the American Toxicodendron. I will only mention two of the most obvious : the first is the white poplar, whose shoots from the lower part of the stem, and the suckers from the root, are garnished with leaves very different in form and size from those on the upper branches, and are covered on both sides in the spring with a woolly down. The next is the willow with smooth leaves, which, if a standard, and the head lopped off, as is usual, the young shoots are garnished with leaves much broader, and of different forms from those on the older branches ; and these have frequently a hairy down on their under surface, which does not appear on those of the older. So that a person unacquainted with these differences in the same tree would suppose they were different. And the American Toxicodendron has varied in these particulars much more, in different seasons, than what Mr. Ellis has mentioned.

Mr. Ellis next says, that the Toxicodendron mentioned by Mr. Catesby, in his Natural History of Carolina, is not the same with that, which is now called by the gardeners poison-ash : but I am very positive of the contrary ; for most of the plants in the nursery-gardens about London were first raised from the seeds, which were sent by Mr. Catesby from Carolina ; part of which were sent to the late Dr. Sherard, as is mentioned by him in the Philosophical Transactions, N^o. 367 ; and another part came to my hands,

hands, from which I raised a great many of the plants, which were distributed, and some of them are now growing in the Chelsea garden.

And that this shrub grows naturally in Carolina, I can have no doubt, having received the seeds of it two or three times from the late Dr. Dale, who gathered them in the woods of that country.

In my paper above-mentioned I likewise observed, that the seeds, which were sent to the Royal Society by Father D'Incarville, for those of the true varnish-tree, did not prove to be so; but the plants, which were raised from them, were taken to be referred to the spurious varnish-tree of Kœmpfer; which I believed to be the same, and own, that it is yet my opinion, notwithstanding what Mr. Ellis has said to the contrary: for the number of lobes or *pinnæ* on each leaf, with their manner of arrangement on the midrib, are the same. And here we must observe, that the figure of this given by Kœmpfer is from a flowering branch; and every gardener or botanist must know, that the leaves, which are situated immediately below the flowers of most winged-leaved plants, have fewer lobes or *pinnæ*, than those on the lower branches: therefore I must suppose it to be the case in this plant; and from thence, with some other observations which I made on the seeds, I have ascribed it to be the wild or spurious varnish-tree of Kœmpfer. But Mr. Ellis is of a contrary opinion, because the base of the lobes of those plants, which were raised from Father D'Incarville's seeds, are rounded and indented like two ears. In Dr. Kœmpfer's figure and description of the *fasi-no-ki*, the leaves are intire, and come to a point at their base.

Here I think Mr. Ellis is a little too hasty in giving his opinion, as he has not seen this plant in the state, that the branch was, from which Kœmpfer's figure was taken. For as there are often such apparent differences between the leaves on the lower branches of trees, and those which are at their extremities, as that in the descriptive titles of the species Dr. Linnæus frequently uses them to distinguish one from another; so in making the same allowance for the plant in question, I cannot help thinking that I am in the right, and must abide by my opinion, till the plants, which have been raised from Father D'Incarville's seeds, have flowered, to convince me of the contrary.

However, I cannot help observing, that Mr. Ellis has given a title to this shrub before he had seen any of the characters, which are necessary to determine the genus. And I have pretty good reason to believe it should not be joined to the *Rhus*; for the three seeds, which I received from the Royal Society, were shaped like a wedge, being thicker on one edge than the other, and not unlike those of the beech-tree, as I noted in my catalogue when I sowed them; and, by their structure, seemed as if the three seeds had been inclosed in the same capsule.

If it proves so, this will by no means agree with the characters of *Rhus*; especially if the male flowers should grow upon different plants from the fruit, which is what I suspect. Nor can I agree with Dr. Linnæus in this particular of joining all the species of *Toxicodendron* to the genus of *Rhus*, many of which have their male flowers growing upon different plants from the fruit; and therefore would
more

more properly come into his twenty-second class of *Dioecia*, than his fifth of *Pentandria*, into which he ranges the *Rhus*. At the bottom of the characters of that genus he has added a note, to shew the varnish-tree is so.

But as there are several other species, which agree in this essential character of distinction ; so, according to the Linnæan system, they should be separated from the *Rhus*, with another generical title.

Mr. Ellis observes, upon the poetical description, which he says Kœmpfer has given of the leaves of the wild varnish-tree turning red in the autumn, that he had not found it to be the case of the tree growing in the stove at Busbridge. How it appeared in that situation, I know not ; but the leaves of all those, which are growing in the Chelsea garden, and stand in the open air, do constantly change to a purple colour in the autumn, before they fall off from the shrub : but those of the true varnish-tree are much more remarkable for the deepness of their colour.

Mr. Ellis says, he had received a letter from Dr. Sibthorp, professor of botany at Oxford, in which the Doctor informs him, that there is no specimen of the true varnish-tree in the Sherardian collection at Oxford ; but that there is one of *jasi-no-ki*, or spurious varnish-tree of Kœmpfer. How the Doctor could write so, I cannot conceive ; for I am very sure there was no specimen of the latter in that collection while it remained in London, having myself often viewed that part of it : and sure I am, Dr. Dillenius never added that synonym to the former : and I do believe the latter was no other way known in Europe, than by Kœmpfer's figure and description of it, ex-

cepting that specimen of Kœmpfer's now in the British Museum.

But, to confirm what I have before said, of Dr. Sherard's having a specimen of the true varnish-tree, I beg leave to quote what Dr. Dillenius has written in the *Hortus Elthamensis*; where, after having described the American Toxicodendron, he says, *Ceterum historiam verniciferæ arboris Japonicæ, diligenter et accurate more suo exsequutus est laudatus Kœmpferius, cujus et descriptio et figura, quin et planta secca, quæ in Japonia lecta servatur in phytophylacio Sherardino, nostræ huic speciei examussim quadrat: id tantum, sexus nempe differentia, prætervisa fuit auctori: quoniam autem ille liber non in omnium his in locis, multo minus in America, manibus versatur, non alienum videtur, si qui, quorum interest, hæc legerint, ut norint, quæ ille de collectione & preparatione vernicis illius habet, hoc loco transcribere.* Then he goes on transcribing from Kœmpfer the manner, in which it is collected.

After this, I find Mr. Ellis is inclinable to think, that the poison-ash, as it is called by the gardeners, is the same with the *fast-no-ki*, or spurious varnish-tree of Kœmpfer. The difference between these shrubs does not consist in small and minute particulars, but the most obvious striking marks of distinction appear at first sight; for the poison-ash has rarely more than three or four pair of lobes to each leaf, terminated by an odd one: in which particular it agrees with the true varnish-tree of Kœmpfer; whereas in the figure, which Kœmpfer has given of the spurious varnish-tree, the leaves have seven or eight pair of lobes terminated by an odd one: and
this

this figure, as I before observed, is drawn from a flowering branch. Every one, who is the least acquainted with these things, knows, that the leaves immediately below the flowers are considerably less than those on the lower part of the branches: therefore this is a more essential note of distinction than those mentioned by Mr. Ellis.

I must also observe, that Mr. Ellis would suggest, that I supposed these two shrubs were only varieties of each other produced by culture: whereas it must appear to every one, who reads my paper, that my intention in mentioning the spurious varnish-tree was to shew it was different from Kœmpfer's true varnish-tree, altho' Kœmpfer supposes otherwise.

For the satisfaction of the curious, I have added a leaf of each shrub, which are now growing in the Chelsea garden, that if any person has the curiosity, they may compare them with Kœmpfer's.

In my paper I took notice, that one of the best kinds of varnish was collected from the *Anacardium* in Japan; and recommended it to the inhabitants of the British islands in America, to make trial of the occidental *Anacardium*, or Cashew-nut tree, which abounds in those islands. This has occasioned Mr. Ellis to take great pains to shew, that the eastern and western *Anacardium* were different trees: a fact, which was well known to every botanist before; and of which I could not be ignorant, having been possessed of both sorts near thirty years. But as I was assured, from many repeated experiments, that the milky juice, with which every part of the Cashew-tree abounds, would stain linen with as permanent a black as that of the oriental *Anacardium*; so I just hinted,

hinted, that it was worth the trial. Nor was my hint grounded on those experiments only, but on the informations I had received from persons of the best credit, who had resided long in the American islands, that people are very careful to keep their linen at a distance from those trees, well knowing, that if a drop of the juice fell upon it, they could never wash out the stain.

But Mr. Ellis, in order to prove that this tree has no such quality of staining, says, he has made some experiments on the caustic oil, with which the shell or cover of the Cashew-nut abounds; and that he found it was not endued with any staining quality. But surely those experiments cannot be mentioned to prove, that the milky juice of the tree has not this property: and Sir Hans Sloane, in his History of Jamaica, says, that the inhabitants of Jamaica stain their cottons with the bark of the Cashew-tree.

I shall not intrude farther on the patience of the gentlemen, who may be present when this paper is read; but humbly crave their pardon for detaining them so long: nor should I have given them this trouble, had not I thought my reputation concerned on the occasion.

LVI. *An Answer to the preceding Remarks.*
By Mr. John Ellis, F. R. S.

Read Jan. 19, 1758. **M**Y letter to Mr. Webb, which is printed in the second part of the xlixth volume of the Philosophical Transactions *, was intended to shew this Honourable Society, that Mr. Miller, in his reply to the Abbé Mazeas's letter, had brought no proofs to lessen the discovery, which he tells us the Abbé Sauvages had made, in attempting to improve the art of painting or staining linens and cottons of a fine durable black colour, by making use of the juice of the Carolina pennated Toxicodendron, instead of the common method of staining black with gauls and a preparation of iron; which, he says, always turns to a rusty colour when washed.

Mr. Miller, instead of producing the proper proofs, to shew that this method of staining cottons and linens of a black colour was known before, or quoting the authors in which he says it is mentioned, contents himself with telling the Society, that this American Toxicodendron is the same plant with the true varnish-tree of Japan; and that callicuts are painted with the juice of this shrub.

In my letter to Mr. Webb, I have endeavoured to shew, that notwithstanding the authority of Dr. Dillenius, and the authors that have followed him, it does not appear, from Dr. Kœmpfer's description of this Japan plant, that it can be the same with our American one.

* Page 806.

The design, then, of this paper, is to lay before this Society some further reasons, why these plants cannot be the same; and that even if they were the same, Mr. Miller has produced no authority to shew, that this juice was ever made use of for this purpose abroad; with some remarks on his reply to my letter, in which he obliges me to be more particular than I intended, in explaining some errors, which I find he has run into.

In my letter to Mr. Webb, I have pointed out the exact description, which Kœmpfer has given us of the leaves of this plant, shewing how much they differ from our American one: but now I shall mention some observations that escaped me before, and which, I think, will give us a clearer proof of this matter.

Kœmpfer, then, informs us, that this Japan varnish-tree, or *Sitz-dsju*, is a tree, not a shrub: and this author (it is well known) is remarkably exact in the description of his Japan plants, making the necessary distinctions between a shrub, an arboresecent shrub, and a tree. He then goes on to explain the manner of its growth; and tells us, that it grows with long sappy shoots, very luxuriantly, to the height of a fallow or willow-tree, which we may reasonably allow to be from 20 to 30 feet: whereas this Carolina pennated *Toxicodendron*, as Mr. Miller tells us in his Dictionary, 6th edit. in folio, is a shrub, and seldom rises above five feet high with us: and many people, who have been in North America, agree, that it is but a slow grower there, and is one of the shrubby underwoods of that country: so that, allowing it to grow even double the height it does
here,

here, it is still but a shrub, in comparison with the other.

Further, while Dr. Dillenius was warm with this supposed discovery, of our having got the true Japan varnish-tree in America, attempts were made there, by intelligent persons under his direction, to procure this varnish after the manner of Kœmpfer; but without success, as I am assured by persons of that country now here, with whom the Doctor corresponded.

Let us now consult the growth of the Carolina and Virginia Sumachs, or *Rhus*'s, in our nursery-gardens, and compare them with this little shrubby *Toxicodendron*, and we shall find, that even in this cold climate nature keeps her regular proportionable pace in the growth of vegetables of the same country.

Let us observe the growth of some of these *Rhus*'s, and we shall find that great luxuriancy of the shoots, which Kœmpfer so justly describes in his varnish-tree. One of these American ones even seems to promise the same height as the Japan *Rhus*; whereas this little shrubby *Toxicodendron* still preserves the same dwarfish slow-growing habit, that it has in its native country.

This leads me, in the next place, to shew, that these two plants must be of different genus's; the one a *Rhus*, and the other a *Toxicodendron*: and if so, according to Mr. Miller, they ought to be properly distinguished, and not ranked together, as Dr. Linnæus has done.

In order to prove this, let us then examine Kœmpfer's description of the parts of the flower, and see
 L11 2 whether

whether it does not answer exactly to the genus of *Rhus*; and whether the flowers are not male and female in themselves, that is, hermaphrodites, on the same tree. The original of Kœmpfer is as follows, p. 791 of his *Amœnitates*: “Flosculos continentes pumilos, et citra coriandri feminis magnitudinem radiantes, in luteum herbaceos, pentapetalos, petalis carnosiss nonnihil oblongis et repandis, staminibus ad petalorum interstitia singulis, apicatis, brevissimis, stylo perbrevis tricuspitate, floris turbini infidente; fructus flosculum excipit gibbosus utcunque in rhomboides figuram compressus.” Whereas Dr. Dillenius, and the authors that have copied after him, say, that his *Toxicodendron* has the male blossoms on one plant, and the female on the other; from whence it must evidently be another genus.

It appears, however, that Dr. Dillenius was not altogether ignorant of this difference of genus in these two plants; but, rather than his *Toxicodendron*, which he had made agree exactly in the leaves, should not agree in the fructification, he makes the accurate Kœmpfer guilty of an unpardonable oversight, in not taking notice of the difference of the sexes of this varnish-tree in different plants: whereas we have just now shewn, that nothing can be more minutely and judiciously described, than he has done both the male and female parts of the blossom, which change into the fruit on the same plant.

The original of Dr. Dillenius's remarks on Dr. Kœmpfer's specimen runs thus: “Planta ficca, quæ in Japonia lecta, servatur in phytophylacio Sherardino, nostræ huic speciei examussum quadrat, id tantum

“ tantum sexus nempe differentia prætervisa fuit auctori.” Hence we find how this error came to spread, and this false synonym to be adopted by the botanic writers, who copied after Dillenius.

This shews us what little dependance we can have upon the result of that meeting, which Mr. Miller mentions he had with his botanic friends; where, from the similitude of leaves only, without the parts of fructification, they determined these two plants, so different in their growth, to be one and the same plant.

Mr. Miller remarks very justly, that the leaves of the same tree often vary much in shape, such as those of the poplar, fallow, &c.

But in answer to this, we may reasonably suppose, that Dr. Kœmpfer, who was on the spot, would not choose for his specimens leaves of the most uncommon sorts that were on the tree, and neglect the most common. This would be carrying the supposition farther than can be allowed, unless we suppose this author had not the understanding even of a common gardener; for otherwise, I am persuaded, Sir Hans Sloane would not have thought his specimens worth purchasing.

For another synonym to the true Japan varnish-tree, as also to Dillenius's pennated Toxicodendron with rhomboidal fruit, Mr. Miller brings in (in his answer to the Abbé Mazeas's letter) the Bahama Toxicodendron *foliis alatis fructu purpureo pyriiformi sparsis* of Catesby's Nat. Hist. vol. i. p. 40. so that he would have all these three different plants one and the same: and, in his reply to my letter, he still insists on it, that these two Toxicodendrons are the same. But

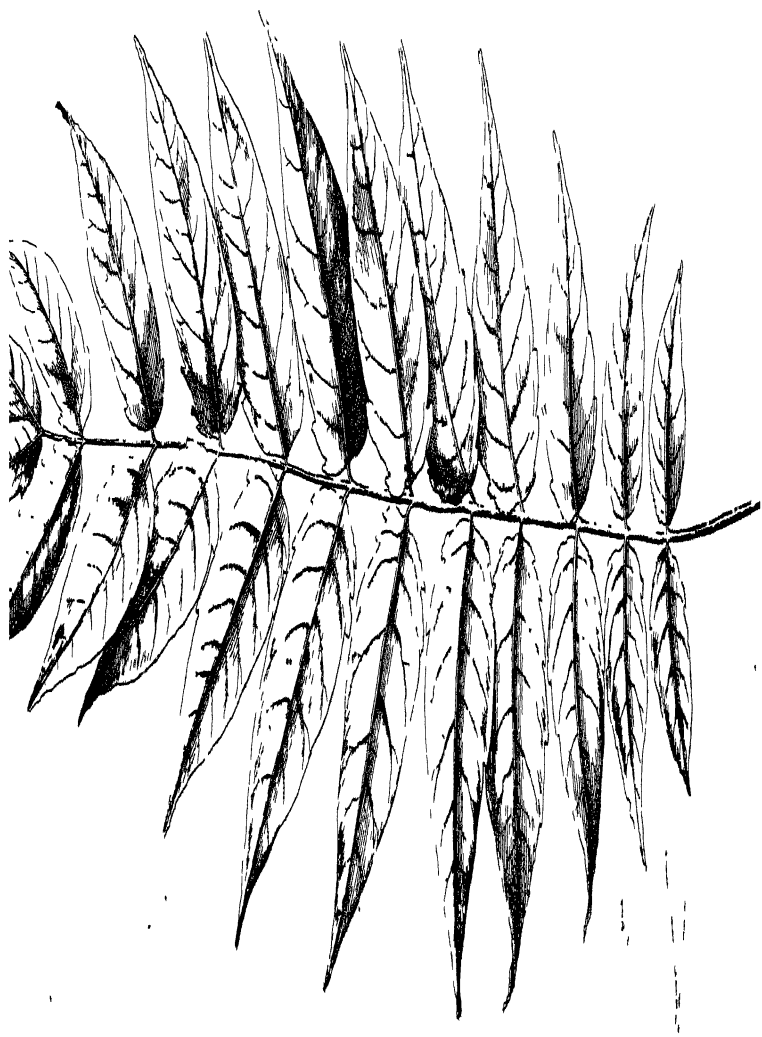
here I must beg the favour of this Honourable Society, when they come more attentively to consider this matter, to compare his answer to the Abbé Mazzeas's letter, and his reply to me, in this particular part.

I shall only at present take notice, that Catesby says, this *Toxicodendron*, with the pear-shaped fruit, grows usually on rocks in Providence, Ilathera, and other of the Bahama islands; and does not mention, that he ever saw it in Carolina. I cannot find it described by any author as growing in Carolina, or in any other part of the continent of North America: nor do I believe that there is a plant of it now growing in England, or that it is even the same genus with Dillenius's rhomboidal-fruited one, from the different structure both of its leaves as well as fruit.

In looking over Dr. Linnæus's *Hortus Cliffortianus*, I find he gives this Bahama *Toxicodendron* of Catesby as a synonym to his *Elemifera foliis pinnatis*, p. 486.

I now come to that part of Mr. Miller's reply, relating to the China varnish-tree, that was raised from seeds sent to the Royal Society by Father D'Incarville; where he still insists on it, that this is the same with the spurious varnish-tree of Kœmpfer. His reasons are, that notwithstanding the indentation and roundness of the bottom of the lobe-leaves of the China varnish-tree, and tho' the lobe-leaves of the spurious Japan varnish-tree come to a point at the base, and are no-way indented, but quite even on the edges; yet he says, because they have an equal number of *pinnæ*, or lobe-leaves, on the whole leaf of each tree, they must be the same.

In



In answer to this, I say their lobe-leaves are not equal ; for I have examined both the specimens and drawings of Dr. Kœmpfer's spurious varnish-tree, and I don't find that the number of the *pinne* exceed seven on a side : whereas I have a small specimen of a leaf by me, that was taken from the top of one of D'Incarville's China varnish-trees, which is above eight feet high, and stands in an open exposure ; and this leaf, tho' but a foot long, has 12 lobe-leaves on a side, and each lobe indented at the base *. At the same time I observed, that the leaves of the young shoots of another tree were a yard long, as they were this summer at the garden of the British Museum. Another thing is remarkable in the leaves of this China varnish-tree ; and that is, the lobes of the leaves, as they approach to the end, grow smaller and smaller ; whereas in the spurious Japan varnish-tree they are rather, if there is any difference, larger towards the end.

I shall make this further remark, that tho' these indentations on the lobe-leaves may vary in number in this China varnish-tree ; yet, as I observed before, since they are continued on even in the smaller leaves at the top of the branches of a tree eight feet high in the open ground, it appears to me, that this specific character, besides the form and intortion of the lobe-leaves, will ever distinguish it as a different species from the *Fusi-no-ki*, or spurious varnish-tree of Kœmpfer.

Mr. Miller now goes on to tell us, he is confirmed in his belief of their being the same, by making some observations on the seeds of this China varnish-

* See TAB. XVII. where this specimen is exactly delineated.

tree; and therefore asserts, that they are the same. It is natural to suppose he compared them with the accurate drawings of the seeds of Kœmpfer's *Fasi-no-ki*, p. 794. that being the only place where the seeds of it are described.

In the very next paragraph Mr. Miller seems to forget, that from his own observations on the seeds of the China varnish-tree, he has asserted it to be the *Fasi-no-ki* of Kœmpfer; but now he finds, in his memorandums, that those seeds were wedge-shaped, and like the seeds of the beech-tree; and that all the three seeds he received seemed to be inclosed in one capsule: so that now he is at a loss what to call it; and at the same time says I have been too hasty in calling it a *Rhus*.

Mr. Miller goes on, and allows this China varnish-tree changes to a purple in the autumn; but not so deep as the true varnish-tree. I suppose he means, by this true varnish-tree, the Carolina pennated *Toxicodendron*; for Kœmpfer has not told us what colour the true varnish-tree of Japan changes to in autumn.

But this is no certain proof on either side of the question, only a corroborating circumstance of the species of a tree: nor should I have mentioned it, but for the manner in which Kœmpfer, with an imagination truly poetical, describes the autumnal beauty of his *Fasi-no-ki*, or spurious varnish-tree. "Rubore suo autumnati quæ viridantes sylvas suaviter interpolat, intuentium oculos e longinquo in se convertit." Even this description would make one suspect it is not the same with the China varnish-tree, which, I am informed, did not turn purplish in the garden of the British Museum till the first frost came

came on: whereas it is well known, that some of the *Rhus*'s and *Toxicodendrons*, particularly the Carolina pennated one, change to a fine scarlet colour in the beginning of a dry autumn, even before any frost appears.

Mr. Miller seems surpris'd, how I should think, that the Carolina pennated *Toxicodendron*, or poison-ash is like the *Fasi-no-ki* of Kœmpfer. I must here acknowlege, at this time, not having seen Doctor Kœmpfer's specimen, I imagined, from the shape of the lobe-leaves (as he has described them) and from the remarkable scarlet colour of both these trees in autumn, that Mr. Miller might be right in what he has advanced; for it was from his authority I took it, depending on the information he gives us in his Dictionary, fol. edit. 6. under the article *Toxicodendron*, where he takes some pains to assure us, that they are the very same plants.

In the next paragraph I find Mr. Miller has intirely mistaken the meaning of one part of my letter to Mr. Webb; which I must recommend to him to read again, and he will find it exactly agrees with his own sentiments. There he will find my opinion is, that notwithstanding the change of soil and situation, this *Sitz-dsju*, or true varnish-tree, and the *Fasi-no-ki*, or spurious varnish-tree of Kœmpfer, are distinct species of *Rhus* or *Toxicodendron*, and will ever remain so.

Mr. Miller now desires me, since I have seen Dr. Kœmpfer's specimens in the British Museum, to declare, whether I think I am mistaken.

In answer to this, and to satisfy Mr. Miller as well as myself, I have been very lately at the Mu-

feum, and have looked very carefully over Dr. Kœmpfer's specimens, and do sincerely think, as did other judges at the same time, that the *Sitz-dsju* is not the same with the Carolina pennated Toxicodendron, nor the *Fasi-no-ki* the same with Father D'Incarville's China varnish-tree.

Mr. Miller informs us, that one of the best kinds of varnishes is collected from the *Anacardium* in Japan.

In answer to this, I must beg leave to shew the Society, that Dr. Kœmpfer does not so much as mention, that this *Anacardium* grows in Japan; but that the varnish, which is collected from it, is brought to them from Siam: and I believe it will appear plainly, from what follows, that there is not a plant of this kind in the kingdom of Japan; for Siam and Cambodia, especially the parts of those kingdoms, where Kœmpfer informs us this **Anacardium* grows, lie in the latitudes of from 10 to 15 degrees north, which must be full as hot as our West Indies: so that it is not probable, that it would bear the cold of the winters in Japan; for Japan lies from the latitudes of 33 to above 40 degrees north, which is about the same parallel with our North American colonies.

I shall now beg leave to lay before the Society that passage of Dr. Kœmpfer, which relates to this dispute, together with my translation of it, that it may be compared with Mr. Miller's translation, which he gives us in his reply to the Abbé Mazzeas's letter, *Philosoph. Transf.* vol. xlix. p. 164. 2d paragraph.

* This is likewise called the Malacca Bean; from its growing in great plenty on that coast, near the equinoctial line.

Dr. Kœmpfer, in his *Amenitates*, p. 793. speaking of the true varnish-tree, says, “ Colitur frequens
 “ in provinciis Tsi-kocko et Figo, in quibus inserti
 “ agris scapi radices agunt et caudices edunt post
 “ triennium vernicem suppeditantes. Optima regi-
 “ onis, quin totius mundi, vernix perhibetur circa
 “ urbem Jassino colligi. Vernicem ceres Japonica
 “ largitur oppido nobilem et pretiosissimam, sed ad-
 “ modum parcam; nec pro operibus, quæ regio
 “ construit, sufficeret, nisi prius cum, *Nam Rak*, i. e.
 “ vernice ignobiliore ex Siamio invec̃ta, pro basi illi-
 “ nerentur. Siamensis vernix promitur in provincia
 “ Corfama, et regno Cambodiæ ex arbore Anacardo,
 “ incolis *Tong Rak*, i. e. *Arbor Rak* dicta, cujus
 “ fructus officinis nostris Anacardium dictus *Luk*
 “ *Rak*, liquor *Nam Rak* appellatur. Perforatus
 “ truncus immisso tubulo, tantâ copiâ fundit liquo-
 “ rem ut Sinæ, Tunquino et Japoniæ pro deliniendis
 “ utensilibus sufficiat, quin jam Bataviam et alia In-
 “ diæ loca vasis ligneis inclusa appellit.”

Which, translated into English, appears to me to be thus :

‘ This varnish-tree is often cultivated in the provinces of Tsi-kocko and Figo: there they plant the cuttings or truncheons in the fields, which take root, and send forth vigorous shoots, which in three years time yield this varnish.

‘ The best varnish of the kingdom, nay, of the whole world, is said to be collected about the city of Jassino. The produce in Japan of this most noble and very precious varnish, is so very little, that there would not be sufficient for the wares made in the kingdom, if they did not first lay on

‘ a ground with an ordinary kind of varnish, which
 ‘ they call *Nam Rak*, and is brought to them from
 ‘ Siam.

‘ This Siam varnish is collected in the province of
 ‘ Corfama, and in the kingdom of Cambodia, from
 ‘ the tree *Anacardus*, called by the inhabitants *Tong*
 ‘ or *Tree-Rak*; the fruit of which is called in our
 ‘ shops *Anacardium*, or *Luk Rak*, and the liquor is
 ‘ called *Nam Rak*.

‘ To collect this liquor, they bore a hole in the
 ‘ trunk, and put in a tube. By this method they
 ‘ get as much of it as is sufficient not only to varnish
 ‘ all the utensils of China, Tonquin, and Japan, but
 ‘ it is even exported in close wooden vessels to Bata-
 ‘ via, and other parts of India.’

The original of Kœmpfer, p. 794. speaking of the
 true Japan varnish, is as follows: “ Prostat non fin-
 “ cera modo, sed et colorata, vel cinnabari nativa
 “ Sinensi, vel terra rubra (quam Batavi antea, nunc
 “ Sinenses advehunt) vel atramenti popularis ma-
 “ teriâ.”

Which I apprehend may be read thus in English:

‘ This varnish is not only sold quite pure, but
 ‘ likewise coloured, and that with Chinese native
 ‘ cinnabar, and a kind of red earth, which the
 ‘ Dutch formerly, but now the Chinese, bring them;
 ‘ and also with the materials that they make their
 ‘ common (or Japan) ink of.’

Mr. Miller translates it thus. (*See p. 164. vol. xlix.*
Phil. Transact.): ‘ This varnish is used without mix-
 ‘ ture to stain black: but the Chinese mix native
 ‘ cinnabar, or a red kind of earth, with it, to make
 ‘ a different colour.’

Here

Here we may observe, that Mr. Miller uses the words staining black; which is not the sense of the author, who, by mentioning the materials of Japan ink, shews, that even in varnishing black it was necessary to use this black mixture.

Further, Mr. Miller says, that the Chinese mix these colouring ingredients with this varnish: but the original plainly says, that the Chinese import them, and the Japanese mix them with . rvarnish for sale.

And in a former part of this letter, p. 162. vol. xlix. Phil. Transf. he says, speaking of this true varnish-tree, that callicuts are painted with the juice of this shrub. But this bare assertion of his, without producing a proper authority, I am persuaded this Honourable Society will never admit as a matter of proof to invalidate the discovery of the Abbé Sauvages.

In looking over one of the numbers of Mr. Miller's Dictionary, under the title of Anacardium, I find he quotes a passage from Dr. Grew, which Sir Hans Sloane has plac'd among his observations on the Cashew-tree, *Hist. Jam. vol. ii. p. 127.* which is, that cottons are stained with lime, and the oil, or mellaginous succus, called Mel Anacardium (but for the account of this Mel Anacardium I shall refer to Parkinson's Theat. p. 1568); and Mr. Miller seems to think it difficult to know which of the Anacardiums is here meant.

One would be apt think, from this passage, and another that follows a little after in the same page of the Hist. of Jamaica, relating to the black dye of the melagoof this nut, that Sir Hans, at the time his history

was published, thought them, as Caspar Bauhin did, of the same genus, but different species; and therefore he has mixt the observations on both together.

For, immediately after mentioning the staining of cottons with this mellaginous succus, Sir Hans says, that the gum is, in faculties and colour, like gum-arabic; and that it is given internally in female obstructions; and that the juice stains linen, which will not wash out suddenly: but he says it is false, that they remain till they flower next year, as Du Tertre asserts.

Sir Hans further quotes, from an anonymous Brazilian author, that the apples stain linen; and that the gum is good to paint and write; and the bark dyes yarn and vessels serving for pots.

And in another place he quotes De Laet, who compiled a general history of America, and who likewise takes his quotation from an old Brazilian author, treating of the trees of Brasil, That the gum of the Acajou is used by painters; the bark is used to dye cotton-yarn and earthen ware. Here I must remark, tho' foreign to our present purpose, that in the original of Laet, what relates to the earthen ware runs thus: "*Et a faire de vaisseaux de terre.*" So that I believe it will appear more probable, that the bark of these trees was used rather to burn earthen ware vessels, than to dye them, as we find these earthen vessels were used to boil their victuals in.

These two quotations from Sir Hans Sloane confirm the former, with regard to the use of the gum; that is, its being fit, like gum-arabic, to be used for water-colours, and to make ink; and that it is the juice of the apple that stains, but this we find is not durable.

Mr.

Mr. Miller has now only the bark of the Cashew-tree left to support his argument. This the above-mentioned Brazilian writers say, that the native Indians of Brasil used to dye their cotton-yarn with; but of what colour no mention is made. And whether this bark is used to give strength to this yarn, as we dye and tan our fishing-nets with oak-bark, or for ornament, is uncertain; for a great deal of this yarn was used in the making their net-hammocks, as well as their coarse garments.

Mr. Miller then introduces Sir Hans Sloane, in opposition to Dr. Browne, whose History of Jamaica I had quoted, to prove that the juice of the Acajou was of the same nature and properties with that of the gum-arabic, and consequently not fit for varnish: whereas it plainly appears from the foregoing quotations, taken from Sir Hans Sloane, that Dr. Browne is right, and agrees exactly in opinion with him.

He then makes Sir Hans say, that the inhabitants of Jamaica stain their cottons with the bark of the Cashew-nut tree. By this, one would naturally conclude, that Mr. Miller has been endeavouring to prove, in opposition to the Abbé Mazeas's letter, that the art of painting or staining cottons of a fine deep black colour, equal to that discovered by the Abbé Sauvages, as described in his experiments on the Carolina Toxicodendron, was practised by the English forty or fifty years ago in Jamaica.

If this was the case, it is something surprising, that, notwithstanding our great intercourse with that island, the callico-printers of England never got intelligence of this valuable secret.

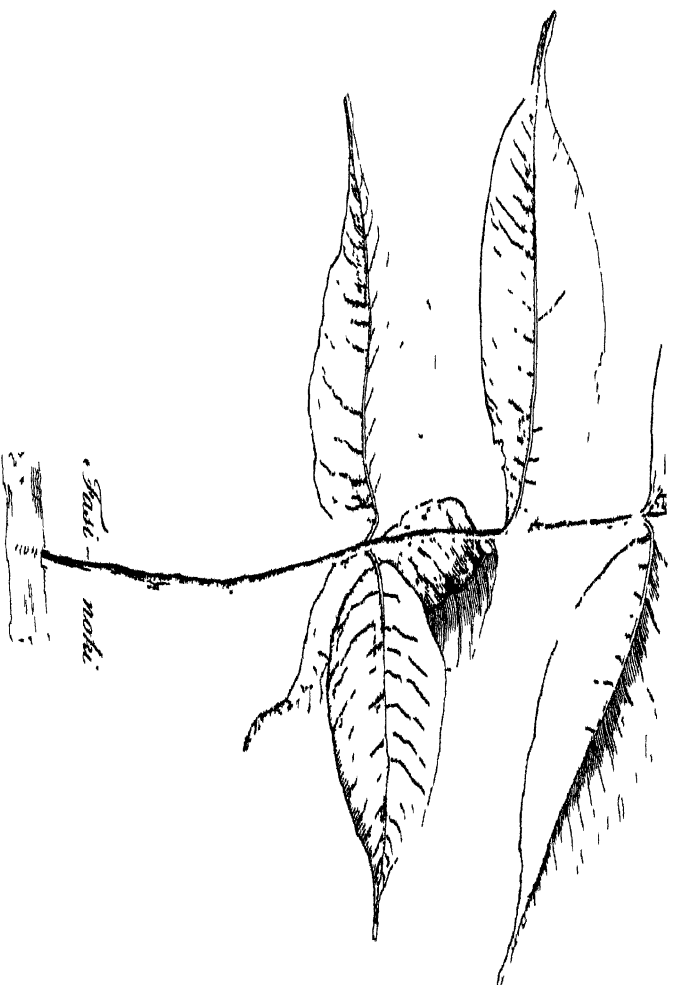
Further, if Mr. Miller will consult Piso and Margrave,

grave, writers of the best authority on the Brazilian plants, he will find their accounts of the Acajou exactly correspond with that delivered by Dr. Browne, in his History of Jamaica, as well as Sir Hans Sloane's: for they say, that the juice of this tree is equal in virtue, and mechanical uses, to the best gum-arabic. And if he still doubts, I shall lastly recommend him to go to the British Museum, and there he may see a most elegant specimen of the Cashew-gum, which will put this matter quite out of all doubt.

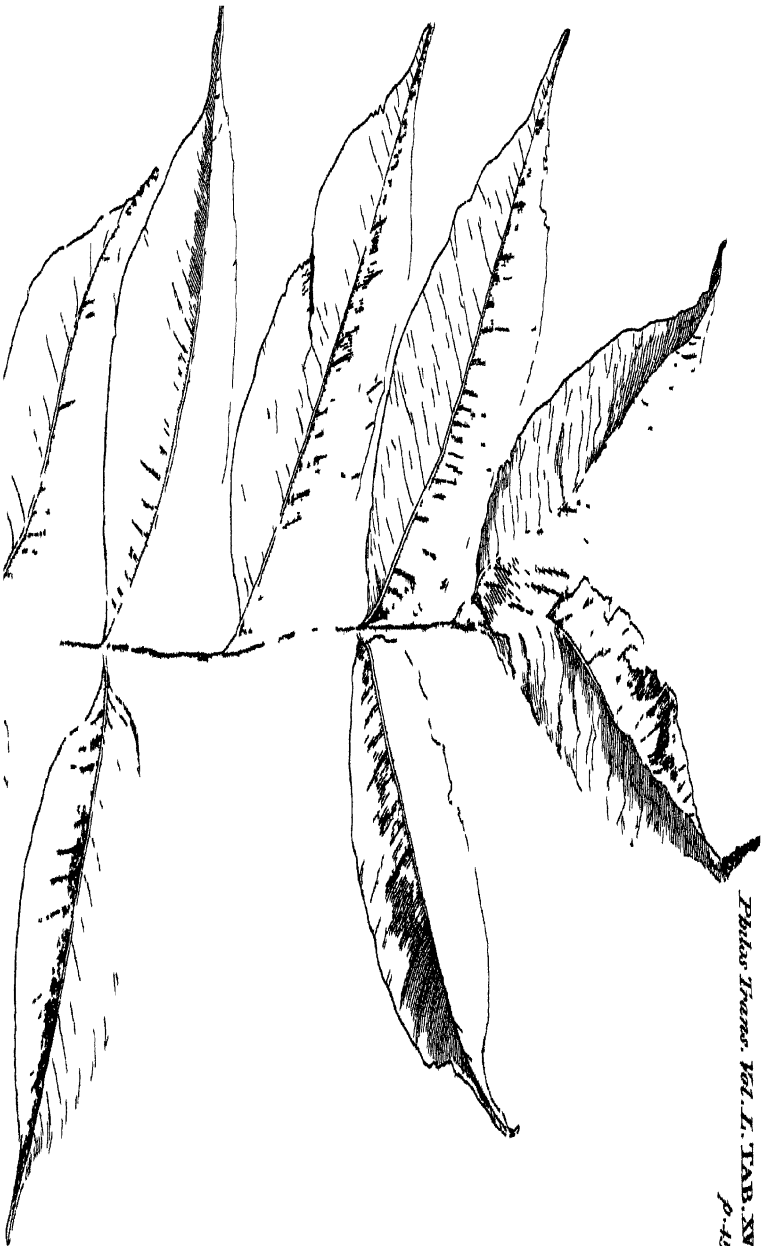
I shall now leave the decision of this controversy, which Mr. Miller has obliged me so fully to explain in my own vindication, to the candour and impartiality of this Honourable Society.

P. S. Since the foregoing paper was read, Professor Sibthorp was so kind to deliver me an exact drawing of the *Fast-no-ki* in the Sherardian collection at Oxford, taken by the Rev. Mr. William Borlase, F. R. S. the title and synonym of which are both in the Hand-writing of Dr. Dillenius, as the Professor assures me. See TAB. XVIII.

B. Borkase Akhu.
J. H. P.



Caraculandrum folios alaba fructu Rhomboides etc. etc. from Sogdiana
the State was Ak-mul-Armi



Philus thunbergii Wed. L. TAB. XVII
p. 456

LVII. *A Letter to the Rev. Thomas Birch, D. D. Secr. R. S. concerning the Number of the People of England; by the Rev. Mr. Richard Forster, Rector of Great Shefford in Berkshire.*

Rev^d Sir,

Shefford, Nov. 9. 1757.

Read Dec. 22.
1757.

SINCE I did myself the honour of writing to you in July †, my book-feller has sent me part ii. of vol. xlix. of the Transactions; in which * I find another medium advanced to determine the amount of the people in England: and this is the number of houses, which pay the window-tax, and which “amount to about 690,000, “besides cottages, that pay nothing.” To this is added, that “tho’ the number of cottages be not accurately known, it appears from the accounts given “in, that they cannot amount to above 200,000.”

Here I cannot but express my concern, that this very ingenious gentleman has not been a little more explicit, by informing us, what these accounts are, upon which he builds so positive a conclusion. The law requires no such accounts to be delivered in; and parish-officers cannot be accused of works of supererogation: besides (which is more to the purpose) I am very certain no such accounts have been given in from this part of the world. On the other hand, in all parts of England, which I have seen (and that is, I think, almost the whole) the number of cottages greatly exceeds that of all other houses, except in the middle of towns, and some villages about Lon-

† See above, p. 356.

* Page 887.

don. This is agreeable to the general interpretation of that sentence passed upon our original parent, that *he should eat bread by the sweat of his brows*; which is, that the majority of his descendents should be poor labouring people. This I do not mention with design to defend the interpretation, but only to shew the general sense of mankind.

As my notion of the matter differs so widely from that of this worthy gentleman, I did every thing in my power to check any mistake, which might arise from a fondness of one's own opinion; and which, I hope, will vindicate me in the eye of every candid inquirer. In a word, I set myself to count all the houses in several contiguous parishes; and then examined how many of them paid the window-tax, or duty upon houses. And here I must observe, that if there be any small mistake, it can hardly be supposed to be in favour of my own scheme; because I had the whole number of houses, by counting as I rode along; and some might possibly be missed, tho' of this I took the utmost care: whereas the number of those, that pay the window-tax, I had from the collectors rolls.

The following table is the fruit of my labours:

Great Shefford	—	90	—	17
Little Shefford	—	12	—	3
Welford	—	162	—	62
Chaddleworth	—	62	—	20
Bright-Walton	—	72	—	21
Catmore	—	10	—	1
Farmborough	—	34	—	5
Fawley	—	47	—	7
East Garston	—	99	—	41
		<hr/>		
		588	—	177

Here

Here we see, that out of 588 houses only 177 pay the window-tax. Now if we say with the philosopher *ex pede Herculem*, and suppose, that 200,000 taxable houses stand in the country, we shall have the following proportion, $177 : 588 :: 200,000 : 664,406$, for the whole number of houses that stand in the country, commonly so called.

Again, Lamborn parish, in which is a market-town, contains 445 houses, of which 229 pay the window-tax. Now if we suppose, in like manner, 200,000 taxable houses to stand in country towns (I mean of the middling and inferior classes), we must then say $229 : 445 :: 200,000 : 388,646$, the whole number of houses, that stand in country towns.

The remaining 290,000 houses must be placed in cities and flourishing towns; and must have Dr. Brakenridge's proportion assigned them; for without all doubt he had some reason for pitching upon such numbers; and as they could not be taken from country towns or villages, must be assumed from the present state of some flourishing place. Upon this supposition, we must say $690,000 : 200,000 :: 290,000 : 84,058$. for the number of cottages in great towns; which, if added to the houses that pay, makes the whole number in large towns to be 374,058. These three sums added together make the total amount of houses in the nation to be

$$\begin{array}{r}
 664,406 \\
 388,646 \\
 374,058 \\
 \hline
 1,427,110 \\
 \hline
 \end{array}$$

[460]

The two former of these numbers should be multiplied by 5, and the latter by 6. The reason of this difference is the great quantity of servants kept in large towns.

$$\begin{array}{r} 1,053,052 \times 5 = 5,265,260 \\ 374,058 \times 6 = 2,244,348 \\ \hline 7,509,608 \end{array}$$

By this way of proceeding it appears, that the whole number of people now alive in England is somewhat more than seven millions and an half. I would not be understood, as if I meant to recommend this as exact; tho' I am in hopes, that, upon trial, it will be found nearer the truth, than any thing hitherto advanced. Neither will I lay any stress upon its approaching so near to the numbers advanced in my former letter; being sensible, that all the methods I have hitherto tried are liable to very great objections. Where certainty may be arrived at by a little industry, all hypothesis should be despised and rejected.

The militia act levies 32,000 men upon the whole kingdom; and in the west riding of Yorkshire 1 in 45, if my intelligence is right, completed their quota. Now if this proportion be applied to the whole nation, $32,000 \times 45$ will give 1,440,000 for the number of balloters; and this multiplied by 5 (which, considering the number of persons excepted, must be under the truth) will amount to 7,200,000 for the total of our people. But I dare not build any thing upon this computation, as many parts of the nation may have heavier quota's laid upon them than the west riding.

Whether the kingdom is really in a declining or increasing state, is, in like manner, a problem not to be solved, I think, by mere calculation. If there happens but a small mistake in the principles, what is built thereupon will be extremely wide of the truth. If one might take the liberty to guess by appearances, I should think we are greatly increased within these forty years, or since the accession of the present Royal Family. This conjecture I found upon the great facility, with which the government raises men, compared to the violent methods made use of in King William's and Queen Anne's time. Indeed I am sensible, that when the great ease, with which the government raises money, and the low interest it pays, have been urged in the House of Commons, as evident proofs of a flourishing trade, and plenty of cash, it has constantly been answered by a gentleman, who understands these matters better than any body else, that they are rather proofs of a want of trade, and that people do not know what to do with their money. In the same manner it may be answered, that the great facility, with which the government raises soldiers, is not owing so much to the great plenty of men, as to the want of employment: which it is possible may really be the case.

But where certainty may be had, it is trifling to talk of appearances and conjectures. For a century now past, the English way of philosophizing (and all the rest of the world is come into it) is not to sit down in one's study, and form an hypothesis, and then strive to wrest all nature to it; but to look abroad into the world, and see how nature works; and then to build upon certain matter of fact. In compliance with this noble method, I have done all
in

in my power: I have examined the registers of several neighbouring parishes, and send you the substance of three of the most perfect ones. Indeed, I could have added several others; but as they seem to have been now and then neglected, I did not care to trust to them. However, this I can safely deduce from them; *viz.* that what I have here sent will be a proper standard for these parts: and if other gentlemen would take the like pains (and it is next to nothing) in four or five parishes in each county, and in every great town, we might perceive, by one cast of the eye, whether our people are in an ebbing or flowing state. I have not set down the burials, as that would but have embarrassed the table; and the increase will appear very well without them. However, upon an average of all the parishes I have examined, the proportion of the burials to the baptisms is as 83 to 149,4.

	Lamborn.	Welford.	Shefford.	Total.
From 1614 to 1623 inclus.	327	67	69	463
1624 to 1633 ———	401	62	64	527
1634 to 1643 ———	391	119	86	596
1662 to 1671 ———	441	146	93	680
1672 to 1681 ———	380	132	108	620
1682 to 1691 ———	451	201	112	764
1692 to 1701 ———	366	134	88	588
1702 to 1711 ———	387	137	84	608
1712 to 1721 ———	422	171	97	690
1722 to 1731 ———	483	156	106	745
1732 to 1741 ———	578	205	128	911
1742 to 1751 ———	566	253	137	956
1752 to 1756 ———	349	120	64	533
				This

This table stands in need of no remarks : it speaks loud enough of itself, that our people increafe in a very rapid manner. All I fhall take the liberty of obferving from it is, that all the registers I have looked over feem to refent the wretched policy of King Charles II. who fubmitted himfelf and kingdom too much to a powerful neighbour : and that our civil war had no effect upon our numbers, in comparifon to our foreign wars.

I truft, that the very ingenious author of the *politico-arithmetical* letters, I have all along had my eye upon, will take no offence, if I recommend an article or two advanced by him to be reconsidered ; which, if purfued, might perhaps induce fome fmall errors in government.

The firft is, That all ways to increafe our people would be for the public welfare, even the naturalizing of foreigners : whereas, if I remember right, all political writers lay it down as a maxim, that numbers of people without employment are a burden and difeafe to the body politic ; and where there is full employment, there the people multiply of courfe. So that we fhould not meafure the happinefs of the nation by the number of mouths, but by the number of hands. Nay, if we were to import a quantity of foreigners, we muft immediately re-export them, as we actually did in the cafe of the Palatines and Saltzburghers. Indeed, I cannot deny, but that if the new-comers were to bring new trades with them, they would be welcome : tho' I apprehend it is not an eafy matter to find out many new manufactures. I can at prefent think of nothing but the cambrick-bufinefs;

business; and that, with a little encouragement, might be established in either Scotland or Ireland, without the importation of strangers.

The next thing I propose to be ruminated is the assertion, That our commerce at sea is one cause of the decay of our fencible men: which sounds in my ear like saying, that if we had less trade, we should have more people. And if this is the purport of it, I am afraid it is a paradox, literally so called.

That emigrations to our colonies do lessen our numbers in appearance, is beyond dispute: but then it is only in appearance: for if employment begets people, the filling our plantations must increase us beyond imagination, it having been made out, if I misremember not, that every man rightly occupied in America finds employment for three persons in Old England. But then care should be taken, that the planters were generally employed in raising rough materials; and that every thing imported there were manufactured by ourselves; because, if we settle colonies, and then supply them with East-India stuffs and foreign linens, it is neither better nor worse than being at a vast expence to maintain other people's poor.

I cannot conclude without begging leave to observe, that this gentleman's doctrine is, from beginning to end, to say the best of it, ill timed. We are contending with our hereditary enemy, the most powerful prince in the world, not for superiority, but for independence, *pro aris et focis*. And, at such a time as this, to be told, that we are but little better than half peopled, and the few we have
dwindling

dwindling away every day, is indeed very discouraging : whereas, on the contrary, I do not balance one moment to declare it, as my fixt persuasion, that we can spare 100,000 brisk young fellows, and still be the most populous flourishing nation in Europe.

I am,

Reverend Sir,

Your affectionate Brother,

and very humble Servant,

Richard Forster.

LVIII. A Letter to the Right Honourable the Earl of Macclesfield, President of the Royal Society, from the Rev. William Brakenridge, D.D. F.R.S. containing an Answer to the Account of the Numbers and Increase of the People of England, by the Rev. Mr. Forster.

My Lord,

Read Mar. 16,
1758.

AS I endeavoured, at a former meeting of the Society, to answer extempore some objections offered by a Gentleman in the country, to what I have wrote concerning the number of people in England ; I now presume to send you what I said then in writing, with some farther reflections. And this subject I never intended to

have meddled with any more; but as I seem to be called upon, to defend what I have formerly wrote, I hope I shall be excused if I briefly attempt it. Your Lordship, I know, and our illustrious Body only desire a fair representation of facts, which is the ground of all philosophical inquiries; and therefore I shall endeavour to do this, as far as I can, without regarding any hypothesis.

My design, when I first entered on this subject, was to discover whether our people were in an increasing or decreasing state, with regard to their numbers; which I thought of great importance to be known, because of its influence on the affairs of Government, in determining our strength, in settling of taxes, and directing us in the œconomy and imployment of our people. Now, in order to proceed in this inquiry, it was evident to me, that if the number of houses were exactly known, the number of people would be nearly ascertained. And therefore I attended to this, to find out the number of houses, as the only thing that could with any certainty help us to judge of this matter. And accordingly, being resolved to depend only upon the most sure, and general observations, I applied to a public office, where I thought I might possibly get at their number. And I there found, that from the last survey that was made, since the year 1750, there were 690,700 houses in England and Wales that paid the window-tax, and the two-shilling duty on houses; besides cottages that paid nothing. By cottages are understood those who neither pay to church or poor, and are, by act of parliament in 1747, in consideration of the poverty of the people, declared to be exempted both from the

tax and the two-shillings duty; and they only remain not accurately known, to ascertain the whole number of houses. However, they are so far known, that from all the accounts that are hitherto given in, they do not appear to be so many as 300,000; and from what I myself have seen, in the books of that office, I should think they were not much above 200,000; for in some places, that I was perfectly acquainted with, I found many of the day labourers rated to the two-shillings duty, and there did not appear to be one house in ten omitted. And therefore, if there are not 300,000 cottages, as seems plain to me, there cannot be a million of houses in the whole in England and Wales; and the rated houses are to the cottages more than two to one; of both which, according to the returns made, there is now about one in seventeen or 58,800 empty throughout the kingdom. But if we were to allow, that there are a million of houses in the whole; which is more than the Gentlemen in the above mentioned office believe, and then deduct those that are empty, there could not be above 941,200 inhabited houses; and consequently supposing six to a house, about 5,647,200 people, or near about five millions and an half; which at the utmost, is what I insist on to be the real number.

But now the Gentleman, who objects to my calculations, thinks, that I have made the number of houses too few, and that in the whole there are above 1,400,000 houses, of which he imagines there are more than 700,000 cottages; for he supposes them to be more than the rated houses; and from thence he infers, that there are about seven millions and an half of people, in England and Wales; which I wish, with all

my heart; was the true number : But I am so far from thinking that I have under-rated them, that I suspect I have rather made them more than they are. However, this controversy will soon be determined, there being now orders given, as I am informed, to all the Officers concerned in the window-tax, to make an exact return of all the cottages, as well as the rated houses, in each of their several districts. In the mean time, the Gentleman and I differ in this, that he supposes above 400,000 cottages more than I can possibly imagine.

Let us now see upon what grounds, and by what method of reasoning he determines his numbers. He makes a division of the 690,000 taxed houses into three classes, placing 200,000 of them in the open country and villages, and 200,000 in the market and inferior towns, and the next, *viz.* 290,000, in the cities and great towns ; for which division he has nothing to direct him ; no proof, nor even probability. And as it is a mere arbitrary supposition, all reasoning and calculations founded upon it are nothing to the purpose, and the number of houses or people computed from thence must be false or uncertain. But yet, upon this supposition, as if it was absolutely certain, he goes on to compute the houses and people in each division.

As to the first, he says he has counted all the houses in nine contiguous parishes in Berkshire, in which he has found the whole number to be 588, and those charged to the duty to be only 177 ; and therefore the cottages are to the rated houses as 411 to 177, or above two to one. And from this he assumes, that the whole number of houses thro' the villages and open country in England will
be

be to the cottages nearly in the same proportion. But here I am surpris'd, that he should reason in so loose and an inaccurate a manner. For, as there may be 7000 parishes in the villages and open country, to infer from the numbers in nine of them that are contiguous, and that all of them together do not make a very large parish, many being much larger as to the number of houses, and where there may be particular circumstances; I say, to infer from them what the proportion will be in all parishes, in the villages and open country, is the same way of reasoning as to say, because the poor in one parish are in such a proportion, therefore they are so in 1000 parishes, or thro' four or five counties: whereas it is plain, that the proportion differs almost in every parish, and in every county; and the sum of all must be added together, before we can know what the real proportion is. And nothing can be inferred from the circumstances of a few parishes, or even of a County, what the proportion will be in the whole. And yet, from such precarious and vague reasoning he presumes to compute, that there are above 460,000 cottages in the villages and open country; having assumed, without any hesitation, that there are 200,000 rated houses in that extent. Such reasoning is unusual in philosophical inquiries.

In like manner the Gentleman reasons very inaccurately about his second division, containing the lesser market and country towns, having supposed in them 200,000 taxed houses: For from one instance of the market town of Langborne, having found the whole number of houses to be to the cottages as 445. to 229, or the rated houses to the cottages as 216 to 229, he supposes the like proportion in all the market.

ket towns. That is, tho' there be perhaps above 300 market towns in England, he supposes each of them has the same proportion of the poor in it as the single town of Langborne; which is unreasonable to imagine. For every one of them may have a different proportion, according to the various circumstances of their trade and situation. But yet from this strange and uncertain way of reasoning, without any induction, and from one instance among 300 cases at least, he concludes by proportion, that there are 388,646 houses in the country market towns, of which there are 188,646 cottages, besides those in the cities and great towns.

In the next place, as to his third class, the cities and great towns, he allows, that my proportion may be among them, *viz.* that the rated houses are to the cottages as 690,000 to 200,000, or 69 to 20: For he thinks, that it cannot be any-where but in the most flourishing places. And therefore, as he has arbitrarily placed 260,000 taxed houses in them, he computes that they must contain 84,058 cottages. But he has given no proof, that my proportion is only in the most flourishing places, besides these few instances that he has produced; which are nothing to form any general conclusion upon. For if we were to be directed by a few cases, we might think that there were much fewer cottages than I have allowed. There are some parishes, in which there are none at all. In the great parishes of St. James's and St George's Westminster, in which there are about 7000 houses, there are none: in the country parish of Chislehurst in Kent, where there are above 100 houses, there are but three: and in many parishes there is not one in 20. So that from particular instances, there is nothing

to be concluded. But in all Middlesex, London, Westminster, and Southwark included, in which the poor are as numerous as in most places in the kingdom, because of the numbers of labouring people that flock hither for employment, there is nearly the same proportion that I have assigned. For from a late survey in that district, as I am informed, there are 87,614 houses in the whole, and of these 19,324 cottages, and 4810 empty. Which indeed shews, that we are not so populous, in and near the metropolis, as is commonly supposed, and much less than I had calculated in my first letter : For from this account, if it be true, there are not above 530,000 people in that compass ; of which, within the bills of mortality, there die about 25,000 yearly ; that is, not less than one in 20.

As to what the Gentleman mentions concerning the militia, he seems to be much mistaken. For if the proportion be as he says, that one in 45 is levied, this directly proves the number of people in England and Wales to be about five millions and an half, according to my calculation ; because the electors or balloters are the fencible men, or those able to carry arms. And if the whole levy be 32,000, then 45 multiplied by 32,000 will give 1,440,000 for all the fencible men in England. But Dr. Halley has clearly shewn, that the fencible men are one quarter of the whole people, children included ; and therefore, four times 1,440,000, or 5,760,000, will be the whole number of the people ; which is nearly what I have made them.

And thus, having seen how he has established his numbers in opposition to me, let us now, in the next place,

place, consider what he has said with regard to the increase of our people. He says, whether the kingdom is really in a declining or increasing state, is a problem not to be solved by calculation: And yet he himself can guess by appearances, that it has greatly increased within these 40 years. But, by his good leave I must tell him, that it is a problem in political arithmetic to be solved from some *data*, as well as others. If the number of people be nearly found, and the general proportion of births to burials, at an average, thro' the kingdom be known, with the annual losses of our fencible men, at a moderate computation; from these *data*, I say, any one, who understands numbers, will easily determine whether we are increasing or decreasing. And accordingly, I have shewn, that the annual increment of our fencible men is not much above 8000, which number is consumed by our annual losses; and therefore we are not in an increasing state. For the whole number of people must always be in proportion to the fencible men; so that, if there is no increase of them, there can be none upon the whole.

It is true, I am the first who ventured upon a solution of this question; but when I consider what I have done, I cannot see but that the principles upon which I reasoned are right. The *data* are, I think, exact enough to discover our state. And Dr. Halley's rule to compute the fencible men, where our losses are to be reckoned, is undoubtedly true. So that if there is any difficulty, it is in fixing the general proportion between births and burials, thro' the kingdom, *viz.* 112 to 100; which I have taken from Dr. Derham, who had collected many observations; being
a greater

a greater proportion than Sir William Petty allowed. And which if it is thought too small, it is to be considered, that within the bills of mortality the births are much under the burials as 4 to 5; and in some of the great towns there are fewer births than burials, and in others they are nearly equal; so that these reduce the proportion that arises from the villages and open country.

But if we were to make a calculation from the births and burials, only in the villages and open country; which Dr. Derham has found to be at an average as 117 to 100, or nearly as 7 to 6; and suppose this to obtain all over Britain and Ireland, in the towns as well as the country, which is surely more than the truth; we shall then find, that the annual increment cannot be more than 9000 fencible men; which corroborates my former estimate. For, to compute it by the principles I have formerly endeavoured to establish; let the number of our people in Britain and Ireland be eight millions and an half, that is, five and an half in England and three millions in Scotland and Ireland; because some Irish Gentlemen have assured me, from some facts, that there is half a million more in their country than I formerly allowed; for I did not pretend to calculate them; and then the annual number of the dead, in Britain and Ireland, being one in 40, will be about 212,500; which will be to the births as, 100 to 117: And therefore the births must be 248,625, and the increase 36,125; of which the fourth part is about 9000 for the fencible men, which I am persuaded is more than the real number.

Now let any one compute our losses in the moderate way that I have done, and he will easily see, that

they cannot be less than this number; and consequently we are far from increasing. And indeed it is evident from the number of empty houses thro' the kingdom, mentioned above, *viz.* one in seventeen, or 58,000, and one in twelve of those that are taxed within the bills of mortality. For it is impossible, if we were increasing, that there could be so many empty; And therefore the appearance of so much building is only the effect of our luxury, requiring larger, more convenient, and more elegant houses, and not caused by our increase.

However, the Gentleman objects to all this, and says, that he has examined the Registers of some neighbouring parishes, and particularly of three that are perfect; and he finds, that the burials are to the baptisms as 83 to 149; which may possibly be the case, as I myself have known it in one parish in the Isle of Wight, where the place is healthy, and people generally marry. But does he imagine that this proportion is general all over England? If so, we should increase in a rapid manner indeed! for then we should double our people in 35 years, if it were not for our losses; which no reasonable man will venture to say. He does not reflect, that in many country places, from their bad situation, there is very little increase, and in some towns none at all, and in others a decrease, continually supplied from the neighbouring country. Within the bills of mortality there are annually 5000 burials more than the births; and consequently, to maintain our numbers here, there must be a yearly supply of 5000; which destroys the whole increase of six or seven counties. And Dr. Derham found, from the accounts he had of country parishes,
that

that in general among them the proportion of births to burials was not greater than 117 to 100, as we mentioned above; so that nothing can be concluded from particular healthy places. The question is, what is the result upon the whole thro' the kingdom? what is the general proportion of the births to burials, from which the increase is to be estimated? and which Sir William Petty says is 111 to 100, and Dr. Derham as 112 to 100. See if he can disprove these numbers by putting together all the different accounts from every corner, among the towns as well as the country; and if he cannot, to argue only from a few instances is nothing to the purpose; for where there is a multitude of different cases, they must all be considered, to arrive at the general truth. But even in the particulars he mentions, he has not completed his argument; for, to make it conclusive, he should have shewn, that, within these last forty years, the time, he thinks, of our great increase, in those parishes the number of houses or people were increased, in proportion almost as the births were above the burials, as 149 to 83: and if that cannot be made to appear, it is plain, that, for all he has said, the annual increase may be constantly consumed by our losses.

And now the worthy Gentleman having endeavoured to shew, from the case of a few parishes in the country, that we are in an increasing state, he proceeds to give me his serious advice in two particulars:

First, That I would reconsider a proposition advanced by me, That all reasonable ways of increasing our people, even to the naturalizing of foreigners,

would be for the public welfare. In answer to which kind admonition I must say, that I have often considered the thing, as far as I can; and I think this may be easily shewn against any political writer, That it is the interest of a government, when they have powerful and dangerous neighbours, to increase their people by all reasonable means, even to the inviting of foreigners, so far as the natural produce of the country can sustain them; and that it is the fault or weakness of an administration not to be able to employ them. And in Britain, where they can have the assistance of the produce of so many large and fruitful countries of their own in America, I will venture to say, that it is an error in their policy, not to endeavour to increase their people; by which they might be more formidable, and perhaps stronger than their grand Enemy. The present King of Prussia has shewn the utility of this within his dominions; by which he has been enabled to make such a figure in Europe.

The *second* thing he admonishes me to reconsider is, That I have supposed our commerce to be one cause of the loss of our fencible men. And who in the world doubts of it, but himself! Do shipwrecks, the disasters and inclemency of the sea, the scurvy, &c. beget people? But he will say, without these we could not have trade, which employs great numbers of our people; and therefore, what we lose, we may gain another way. And just so he may say of our wars, that occasion the destruction of so many of our people, that they are no loss to us; for we gain by them in their consequences, in securing of our liberties and property, and by which
our

our trade is preserved and promoted. But notwithstanding this, can it be said, that war does not diminish our sensible men ! The truth is, trade increases riches, and gives more of the conveniencies of life, and brings luxury along with it ; but it does not necessarily breed people : For we see in those countries where they have little trade, the people increase much faster than they do with us, as appears from the Bills of mortality in Prussia ; where the general proportion of the births to the burials is greater than it is here, *viz.* 4 to 3 ; and by which the people might double in 84 years, if it were not for their losses. (*Vid. Phil. Transf. vol. xxxvi.*) Which great increase, by the way, easily accounts for those vast swarms of people that came from thence and the adjacent countries in former ages, and over run all Europe. And therefore it is not so terrible a paradox, as he imagines, that possibly where there is much less trade the people may increase faster ; for luxury and other vices, that come with trade, do not promote an increase.

And now, as he has been so good as to give me his advice, I will return the favour, and desire him to reconsider the method of reasoning by induction ; which may possibly help him to escape some paralogisms, in arguing upon these subjects. And I would likewise recommend it to him to inquire diligently, whether the number of our houses in England be increased these last sixty years ; which, according to his reasoning, ought at least to be doubled : For if there is no increase of the houses, there can be none of the people.

To conclude : He adds, that my doctrine, from
beginning

beginning to end, to say the best of it, is ill-timed, when we are contending with our hereditary enemy, *pro aris & focis*. But here his zeal hurries him on, that he does not look to the dates of my Letters. For the first three were read before the Society, and ordered to be printed, long before the war was proclaimed; and as for the last, it is only a supplement to the rest; in which I have shewn, that France, by the bad œconomy of her people, is not in an increasing state; which, I think, is a comfortable hearing. But supposing they had been all printed during the war: What then? Is a fact to be concealed that, if discovered, may be useful to prevent errors in government, and rectify our notions of the œconomy of our people? What advantage can our enemies make of such a discovery? Will it encourage them to imagine that we shall be easier subdued, when they know, by the most moderate computation, we have at least two millions of fencible men in our British islands. Enough, surely, to resist them in all their attempts! But I doubt we are not so deficient in our numbers as in public virtue, without which the greatest multitude may be easily overcome.

And thus, my Lord, I have endeavoured to answer what this Gentleman has wrote in his second Letter; for I pass over the first, as it does not seem to contain any more in opposition to me, than what I have here considered. And upon the whole I cannot see, that he has said any thing to invalidate what I have formerly advanced. If I could discover it, I should be very ready to acknowledge my error.

I am sensible I have made this reply too long ; but
I trust your usual benevolence to all our worthy
Members will excuse me, who shall always esteem
it an honour to be,

My Lord,

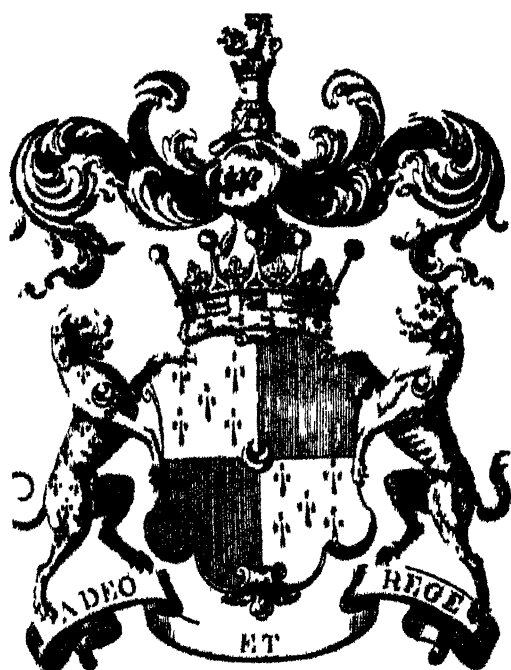
Your Lordship's

Sion-College,
March 16. 1758

Most obedient

and faithful Servant,

Wm. Brakenridge.



Philip Carl. Hanover.

